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THE BRITISH
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THE HIPP CHRONOSCOPE WITHOUT ARMATURE SPRINGS.

BY KNIGHT DUNLAP.

From the Psychological Laboratory of the Johns Hopkins University.

1. *The advantage of working without the springs.* 2. *The method of operating.* 3. *An arrangement of chronoscope and fall-hammer for tests of the method.* 4. *Details of the operation of the instruments.* 5. *The results of the tests.* 6. *Some general conclusions.*

1. As the Hipp chronoscope is usually operated, the armature is moved in one direction by one of the magnets, and in the other direction by one of the springs. This arrangement is adhered to even when both of the magnets are used[1]. A small change in the strength of the current, or in the tension of the springs, produces a variation in the reading of the chronoscope. The current may be kept practically constant by employing storage batteries, but the spring-tension changes with temperature variations and with use, so that constant and elaborate control is needed when the chronoscope is used for reaction work.

The springs may be entirely removed, and the armature moved by the magnets alone. This method of operation does away with the most important sort of temperature effect, and also permits a relatively large variation in the current strength, if the mechanism is properly adjusted. Since the two magnets are nearly alike in windings and cores, if the current through the one is equal to that through the other, the latent periods of the two are practically equal, and hence the actual lengths of the periods are of slight importance.

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[1] See Titchener, *Experimental Psychology, Student's Manual, Quantitative*, 155, 156.

2. In using the chronoscope without springs, two circuits are arranged, one through each magnet, which are independent except that they may both be taken from the same source (battery or power circuit). The circuit through the upper magnet (which we will designate C_1) is controlled by the stimulus-mechanism, and the circuit through the lower magnet (C_2) is controlled by the reaction key. When both circuits are closed, the armature will stay either up or down; if it is up, and the first circuit (C_1) be momentarily broken, it will move down, and the hands of the chronoscope will begin to move; if the second circuit (C_2) be next broken (either momentarily or permanently), it will again move up, and the hands will stop.

Unfortunately, the Hipp chronoscope is so constructed that the action of the armature lever on the clutch-shaft is not positive in both directions. The lever pushes the shaft out of clutch when the armature moves up; but when the armature moves down the shaft is pushed into clutch by a flat spring. To make the action of the lever on the shaft positive in both directions would require an important change in the structure of the chronoscope, and I have therefore worked so far with the instrument with the flat spring.

Having removed the armature springs, I adjusted the counterpoise so that the shaft would stay as well in clutch as out of clutch when no current flowed through the magnets. This adjustment, of course, involves the overbalancing of the armature by an amount sufficient to compensate for the force exerted by the flat spring, and is inferior to the adjustment which might be made if the flat spring were removed. The original counterpoise of the instrument was not sufficiently heavy to balance the armature and spring, and I therefore replaced it by a heavier one.

3. The chronoscope was connected with the fall-hammer and with the power circuit in the following way. Two terminals of a double-pole knife-blade switch were connected with the terminals of the direct current circuit (nominally at 120 volts, but actually varying between 116 and 118 volts) through the resistance of a bank of incandescent lamps. Normally, three 16 c.p. (50 watt) lamps in parallel were used. The same two terminals of the switch were also connected across by a shunt circuit containing a G.-R. sliding rheostat of 15·4 ohms resistance having a continuous load capacity of 2·7 amperes. A voltmeter reading to 120 volts by 1 volt divisions was also connected to

these two terminals, giving the voltage as reduced by the rheostat in the shunt circuit. Under the conditions described, current flowed through the rheostat whether the switch was open or closed, so that temperature changes in the rheostat were minimised.

The other two terminals of the double-pole switch were connected to the chronoscope and the fall-hammer through the two parallel circuits (C_1 and C_2) above referred to. The first circuit (C_1), through the upper chronoscope magnet, had two branches, one controlled by the movable break-key of the fall-hammer, and the other controlled by the make-contact of a relay. The second circuit (C_2), through the lower magnet of the chronoscope, was controlled by the fixed make- and break-key of the fall-hammer, the break-contacts being used. The magnet of the relay was in an independent circuit containing a battery and controlled by the movable make- and break-key on the fall-hammer, the break contacts being used. Both C_1 and C_2 led off from the double-pole switch through an ammeter, reading to 1 ampere by divisions of ·01 amp., so that the current on either circuit alone, or on both together, could be read. When the switch was open, no current flowed on the chronoscope circuits.

4. The instruments as connected were used to test the effect of various current conditions on the chronoscope. The hammer was raised, and caught by its magnet; the hammer keys were raised; the double-pole switch was closed; the chronoscope was started; the hammer was allowed to fall; the chronoscope was stopped; the switch was immediately opened; and the reading was taken. In beginning a series of tests, it was necessary to push up the armature with the finger before closing the switch, or else to open momentarily the circuit through the lower magnet after closing the switch, in order to make sure that the armature was in the upper position. This precaution was not necessary again, as the armature automatically returned to the correct position.

In falling, the hammer performed three functions successively: (1) it opened the first movable key, breaking the first circuit (through the upper magnet of the chronoscope), which permitted the lower magnet to pull the armature down; (2) it opened the second movable key, breaking the battery circuit through the relay magnet, thus permitting the relay to re-make the circuit through the upper chronoscope magnet; and (3) it opened the fixed key at the bottom of the fall, breaking the circuit through the lower magnet of the chronoscope, which permitted the upper magnet to pull the armature up.

It was quite possible to make the second fall-hammer key re-complete the first circuit (C_1) directly, thus dispensing with the relay. In this case, the key needed a spring stronger than the one supplied by the maker, or else the circuit was broken repeatedly by the vibrations resulting from the impact of the hammer on the cushion at the bottom of the fall[1]. A heavy rubber band worked well enough, but the tension was not constant; and this inconstancy is important since it affects the resistance encountered by the hammer in its fall. By using the relay, it was possible to make all three keys act as break-contacts, in which case the jar has no effect.

The fall-time (the interval between the break of the first circuit and the break of the second circuit) was measured by means of the Pfeil marker writing beside a 250 vibration fork. In making these measurements, the connexions were so arranged that the circuit through the Pfeil was broken by the first key, re-made by the relay, and again broken by the third key. The intervals were thus measured from break to break of the circuit; the Pfeil under this condition gives accurate results. The interruption-time (the interval between the breaking of the circuit by the first key, and the remake of the same circuit by the relay) was measured on the same record: the interruption-time was therefore measured from break to make, giving an accuracy within two sigma (thousandths of a second), which is sufficient for this interval.

5. The results of some of the tests are summarised in the table which appears below. The first column (S) gives the reference letter and number of the series. The second column (A_1) gives the ammeter reading when one circuit (C_1 or C_2) was closed. The third column (A_2) gives the ammeter reading with both circuits closed. The fourth column (Ir) gives the duration of the interruption of the circuit C_1 (through the upper magnet). The fifth column (T) gives the duration of the fall-time, that is, of the interval between the break of C_1 and the break of C_2. The sixth column (Ch) gives the average of ten consecutive readings of the chronoscope. The seventh column (mv) gives the absolute mean variations of these readings. The values in the second and third columns are expressed in decimal parts of an ampere; the values in the fourth, fifth, sixth, and seventh columns are in sigma.

---

[1] See this *Journal*, IV. 49. Variations in the tension of the spring on the key at the end of the stroke have no effect on the hammer-time.

S	A_1	A_2	*Ir*	*T*	*Ch*	*mv*
I *a*	·3	·495	50	134	130·5	·5
b	·2	·365	50	134	130·5	·9
c	·1	·195	50	134	131·2	·84
II *a*	·1	·195	68	185	180·3	1·4
b	·3	·495	68	185	180·2	·48
c	·2	·365	68	185	180·9	·58
d	·4	·7	68	185	180·2	·84
III *a*	·2	·365	68	185	182·2	·96
b	·3	·495	68	185	181·8	·78
c	·2	·365	68	185	180·9	·36
d	·3	·495	68	185	180·0	·2
IV *a*	·4	·7	31	58	54·8	·54
b	·2	·365	31	58	55·9	·36
c	·3	·495	31	58	55·0	·2
d	·1	·195	31	58	57·0	1·2
V *a*	·3	·495	73	543	537·6	1·0
b	·2	·365	73	543	536·9	·76
c	·1	·195	73	543	537·1	2·08
VI *a*	·2	·365	20	99	96·7	·42
b	·2	·365	20	99	98·3	·56
c	·2	·365	20	99	96·5	1·0
d	·3	·495	20	99	95·8	1·68

In series III*a*, III*b*, and VI*b*, the magnets were so connected that unlike poles were opposed. In all other cases like poles were opposed. In series III*c* and III*d*, the current through both magnets flowed in the reverse direction from that in which it flowed in series under I, II, IV and V, and in *a*, *c* and *d* under VI. In series VI*c* and VI*d* the original (lighter) counterpoise was used, by which the armature was not held in approximate equilibrium when no current flowed. In these last two series, therefore, the armature had to be placed in the upper position each time, by opening C_2 momentarily after closing the main switch.

In addition to the series from which were obtained the results in the table, fifty series were taken from which data were obtained bearing upon (1) the effects of variation in the current; (2) the effects of reversal of the current; (3) the effect of connecting the magnets with like or unlike poles opposed, and (4) the effects of varying the open circuit voltage on the chronoscope circuit. For this last purpose, rheostats were placed in the circuits C_1 and C_2, so that the current might be kept constant although the voltage was varied. The data from these series were not tabulated, because the fall-times of the hammer were not determined. On the first three points, the results agree perfectly with the tabulated results.

With the properly adjusted and properly connected chronoscope, a change from ·3 ampere to ·2 or ·4 ampere (measured with single circuit closed) does not produce an important change in the readings. In nearly every case in the table, ·3 gives the smallest *mv*, but this is not true of the untabulated results. The mean variations are, however, all below 1 sigma. Current of ·1 ampere gives a deviation of more than 1 sigma from the ·3 ampere readings, and the mean variations are too large to be permissible. If a normal current of ·25 to ·3 amperes is used, a deviation of 20 per cent. from the normal is of no consequence. To produce a change of 20 per cent. in the current of the chronoscope circuit, with the arrangement described, would require a variation of nearly 20 per cent. in the voltage of the power circuit, whereas a variation therein of 4 per cent. is quite abnormal.

It does no harm to reverse the current through both chronoscope circuits, although it is quite unnecessary. In the untabulated series, pairs of series in which the conditions in the two members of a pair were exactly the same except that the current direction in one was opposite to the direction in the other, gave averages of 67·9 and 67·9, 68·0 and 68·3, 68·5 and 68·2. The mean variations were ·54, ·46, ·46, ·64, ·50, and ·78.

The connexion of the magnets with unlike poles opposed gives in every case a mean variation larger than that given by the connexion with like poles opposed. The differences in the table are all small (compare II*b* and III*b*, II*c* and III*d*, VI*a* and VI*b*), but in the untabulated results the mean variation for the first arrangement always gives a mean variation greater than 1 sigma.

I have not found that the voltage is important. Series taken with open circuit voltages of 20, 30, 40, 100 and 115, with the current on a single circuit kept at ·3 ampere throughout, gave a difference of only ·6 sigma between the highest average and the lowest, and no discoverable connexion between order of averages and order of voltages.

6. In using the chronoscope without armature springs for timing reactions it is necessary to balance the armature properly, and carefully to adjust the armature stops and the screw which bears against the clutch shaft. The distance through which the shaft moves from one direction into engagement should be exactly equal to the distance through which it moves from the other direction to release, and the armature should approach as near to the poles of one magnet as it does to the other. I made these adjustments quite roughly, and yet the accuracy is great enough for any reaction work. A short series of control experiments will show the degree of accuracy attained by any adjustment of the chronoscope, and the range of variations in

current which may be permitted. The table of my results shows that in addition to the error due to the fact that the chronoscope reed runs a trifle slow, there is a constant error which may be due to the armature approaching a little nearer to the upper magnet than to the lower, or to the clutch not engaging at exactly the right point, or (what is more likely) to the balancing of the flat spring by extra weight.

Any form of break-circuit key may be used for the reaction key, with the same results, as far as the mechanical action is concerned. No latch or spring is needed to keep the key open, for the reactor cannot possibly release it soon enough to interfere with the proper action of the magnets. The only requisite is that the key shall be closed in time for the next reaction.

The first circuit, through the upper magnet, must be broken at the instant the stimulus is given, and must remain open for a period whose maximum and minimum I have not determined. Certainly, for reaction work, limits of 20 and 50 sigma are perfectly safe. The arrangement of apparatus for visual experiments, and for auditory experiments in which a telephone click is used, is relatively simple. For use in auditory experiments in which the intensity of the stimulus needs to be accurately controlled, I have in process of construction a sound hammer which will break a circuit for a determinate length of time. There is no sort of reaction time work with the Hipp chronoscope in which the instrument may not be used advantageously without armature springs.

(*Manuscript received September* 15*th*, 1911.)

PSYCHOLOGICAL THEORIES OF THE HORIZONTAL-VERTICAL ILLUSION.

By C. W. VALENTINE,

Lecturer in Psychology, etc., to the St Andrews Provincial Committee for the Training of Teachers.

A. INTRODUCTION.
  - *I. Wundt's eye-movement theory.*
  - *II. Lipps's aesthetic theory.*
  - *III. Theory of contrast with shape of visual field.*

B. EXPERIMENTAL INVESTIGATION OF THEORY OF CONTRAST WITH SHAPE OF VISUAL FIELD.
  - *I. Principle and method of the experiments.*
  - *II. Explanation of tables and results.*
  - *III. Experiments by method of mean error.*
  - *IV. Conclusion from both methods.*

C. LIPPS'S AESTHETIC THEORY OF THE ILLUSION.
  - *I. The doctrine of aesthetic Einfühlung as applied to geometrical figures.*
  - *II. Experimental investigation of Lipps's theory.*
  - *III, IV & V. Method and results.*
  - *VI. Aesthetic Einfühlung in school children.*
  - *VII. The 'standing' and the 'hanging' vertical.*
  - *VIII. Conclusion as to Lipps's theory.*

D. SUMMARY OF RESULTS.

A. INTRODUCTION.

THE general tendency to overestimate the length of a vertical line as compared with that of a horizontal of the same length, is an allusion well known to psychologists and also to teachers of drawing, but the cause of it still remains a matter of dispute.

I. *Wundt's Eye-movement Theory.* The most important research upon the subject is that of Dawes Hicks and Rivers[1]. They put to the test the theory of Wundt, according to which verticals are overestimated because vertical movement of the eyes requires greater effort than horizontal movement, owing to the arrangement of eye muscles. Drs Rivers and Dawes Hicks, however, shewed that the illusion is, on the average, just as great when the figure judged is only exposed $\frac{1}{50}$th of a second (quite inadequate for any movement sufficient to form a basis of judgment) as when the exposure is prolonged indefinitely. Besides Wundt's theory, we find two others that have been suggested.

II. *Lipps's Aesthetic Theory.* Lipps[2] ascribes the illusion to the fact that vertical lines are suggestive of activity. They raise themselves up against the force of gravity, are characterised by a certain "liveliness" not found to the same extent in horizontals. This gives to the vertical a certain superiority and leads to our overestimation of its height. With this theory we shall deal later.

III. *The Theory of Contrast with the Shape of the Visual Field.* Külpe[3] points out that the visual field, whether monocular or binocular, and whether for the resting or moving eye, is smaller in the vertical direction than in the horizontal. Apparently the suggestion is that by contrast with the longer field of vision, the horizontal will be shortened as compared with the vertical, which has a smaller field to contrast with.

B. EXPERIMENTAL INVESTIGATION OF THE THEORY OF CONTRAST WITH THE SHAPE OF THE VISUAL FIELD.

I. *Principle and method of the experiments.* If this theory be true, the illusion should be less when only *one* eye is used in the perception of the lines than it is for two eyes; for the closing of one eye cuts off part of the horizontal field of vision, whilst leaving the vertical length unaltered.

Dr Rivers suggested that I should put this theory to the test experimentally by finding the amount of the illusion when both eyes are used by the subject, comparing it with the amount of illusion when only one eye is used. I should like to take this opportunity of thanking Dr Rivers for this suggestion and for his great kindness in looking over my results preparatory to publication, and for the advice and encouragement which he always so readily gives.

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[1] This *Journal*, II. 243.

[2] See article "Aesthetische Factoren der Raumanschauung" in *Beiträge*, published as *Festgruss* to Helmholtz, 1891. Also *Raumaesthetik und geometrisch-optische Taüschungen*, Leipzig, 1897.

[3] *Outlines of Psychology*, trans. by E. B. Titchener, 1895, p. 366. Külpe throws out the suggestion without definitely adopting it as a theory.

Experiments were begun in the Psychological Laboratory at Cambridge in the summer of 1909, with the same apparatus and by the same method as were used by Dawes Hicks and Rivers[1]. The horizontal-vertical figure was exposed for a fraction of a second on a ground-glass screen. The subject, seated at a convenient distance, was warned of the coming exposure and was allowed to express his judgment in any of these forms—"Vertical (or horizontal) much greater," "Vertical greater," "Vertical greater or equal," "Equal." Any constraint of the subject's attention towards one particular line was avoided by leaving him quite free to refer either to the horizontal or the vertical line in expressing his judgment.

On the glass screen a small fixation mark was located at the junction of the two lines. Special care was taken to arrange the subject's position so that this fixation point would be in a line midway between the eyes, when both eyes were used, and exactly opposite the eye in use when only one was in use. This was important, as otherwise slight variations might conceivably have been caused by the different angles of the line of vision.

All the subjects tested were in entire ignorance of the purpose of the experiments and even of the existence of the usual illusion. At each sitting a series of tests would be given with two eyes and then the same series repeated with one eye only. At the next sitting the test with one eye would come first, to equalise any effects of practice or fatigue. The right and left eyes were used alternately. Care was taken to bandage the unused eye so that all light was excluded. Thus not only was the figure invisible to that eye but there was no part of the peripheral field of vision of the bandaged eye exposed, which might conceivably have added to the supposed effect of contrast, produced by the visual field. The subjects were told to keep the bandaged eye open in order to avoid the associated narrowing of the palpebral fissure of the eye in use.

For each subject a few preliminary observations were made to ascertain the approximate amount of the illusion and then the length of the variables to be used in his case were determined accordingly.

---

[1] This *Journal*, II. 245, fig. 1.

II. *Explanation of Tables.* Table I shews the results of the first subject[1]. The results for each standard are sorted out. The numbers refer to the judgments given in reference to the variables. Thus, taking the first line of the judgments with two eyes, we find that the vertical of 40 mm. was twenty-seven times judged to be greater than the horizontal of 40 mm., twice equal to it, and once less than the horizontal. As a rule the variable which is taken to be equivalent to the standard is that figure at which the number of 'greater' judgments most closely approximates to the number of 'less' judgments for the

TABLE I. *Subject W. A.*

(*a*) Standard Horizontal 40 mm.

	Two Eyes				*One Eye*		
Length of Vertical	Greater	Equal	Less	Length of Vertical	Greater	Equal	Less
40 mm.	27	2	1	40 mm.	21	3	6
39·5 ,,	16	9	8	39·5 ,,	18	3	9
39 ,,	13...........	7............	10	39 ,,	16	5	9
38·5 ,,	7	2	21	38·5 ,,	9...........	10...........	11
38 ,,	6	7	17	38 ,,	3	7	20
37·5 ,,	1	2	27	37·5 ,,	5	4	21
Totals...	70	29	84	Totals...	72	32	76

(*b*) Standard Vertical 40 mm.

	Two Eyes				*One Eye*		
Length of Horizontal	Greater	Equal	Less	Length of Horizontal	Greater	Equal	Less
42 mm.	22	4	4	42 mm.	17	5	8
41·5 ,,	21	7	2	41·5 ,,	11	6	13
	....	....	....		....	....	....
41 ,,	8	9	13	41 ,,	10	9	11
40·5 ,,	6	4	20	40·5 ,,	8	5	17
40 ,,	2	7	21	40 ,,	6	7	17
39·5 ,,	2	4	24	39·5 ,,	2	5	23
Totals...	61	35	84	Totals...	54	37	89

The dotted line indicates the point at which it is estimated that we have reached a variable which is, on the average, the equivalent of the standard as judged by our subject. The lower the dotted line, the greater the illusion.

---

[1] By means of a reducing lens alterations as small as 0·5 of a mm. (in later subjects 0·75 mm.) could be made in the lines seen on the glass screen, though the operator only had to make adjustments of 1 mm. on his sliding scale.

same variable. But sometimes one figure is clearly too small and the next one too large, in which cases the dotted line is drawn between as in Table I (*b*) 'Two Eyes.' Also in such a case as in Table I (*b*) 'One Eye,' where horizontals of 41 mm. and of 41·5 mm. give approximately an equal number of 'greater' and 'less' judgments, the mean between the two is taken as equivalent to the standard[1].

It will be seen from Table I that for subject W. A. the illusion, so far from being less for one eye than for two, as it should be if the theory suggested by Külpe holds good, is actually slightly greater for one than it is for two eyes when, as in (*a*), the horizontal is the standard, whilst it is practically the same when the vertical is the standard as in (*b*).

Closer examination of the results revealed a further interesting fact. The number of judgments shewing the illusion in the right eye was considerably greater than that in the left. When the cases of *negative* illusion[2] are subtracted the actual figures are:

	Right Eye	*Left Eye*
Numbers of case of illusion	105	73

Thus early in the course of the research I was led to conclude that the illusion might be different according to the eye used—a conclusion confirmed by later observations. As a result of this observation in the case of W. A., in all further experiments I arranged the same number of tests for each eye individually as for the two eyes, so that henceforth results for the right eye and left eye will be given in separate columns.

Owing to my removal from Cambridge to St Andrews in September 1909, I was unable to continue experiments with the same apparatus. Whilst waiting for the construction of similar apparatus, I tested three subjects by the method of mean error. The actual results are given later, but I may say here that in two cases the illusion was greater both for the right eye and the left taken separately than it was for the two eyes together, and in the other case the illusion for two eyes was intermediate in amount between that for the right eye and that for the left. None of these subjects, then, support the theory of contrast with the visual field.

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[1] In a few doubtful cases later, recourse may be had to the more elaborate method of finding the two figures at which the number of 'greater' judgments and half number of equal judgments given for the same line equals the number of 'less' judgments and half its corresponding number of equal judgments. The variable which is equivalent to the given standard is then taken as the mean between these two figures. In the case just considered (Table I (*b*) 'One Eye') this method would give identical results with the simpler procedure. For 17 'greater' + $\frac{8}{2}$ (given by 42 mm.) = 17 'less' + $\frac{8}{2}$ (given by 40·5 mm.). And the mean would be 41·25, which has already been selected.

[2] By 'negative illusion' is meant the overestimation of the *horizontal*.

Eventually I was able to have suitable apparatus added to the Psychological Laboratory at the Training College, Dundee[1]. It was ably constructed by Mr A. Jenkins, Manual Instructor at the Training College. On this apparatus there was no pendulum as provided in the apparatus used by Hicks and Rivers, but an exposure, almost instantaneous, was obtained by the movement of a paper screen which covered the lens. No subject detected the exposure of one part of the figure before the other; those tested indeed were unable even to say in which direction the screen was moved[2].

My next subject, C. O. K., wore glasses. I performed one set of tests while the glasses were worn and another set without glasses; my reason for this will be given in a subsequent paper. The results are given in Tables II and III. It is interesting to note that this subject, in the course of 2520 judgments, did not once give the judgment 'Equal.' Further, her results are remarkably consistent, only one slight reversion taking place (Table III (*a*) Left Eye), despite the fact that this subject and the operator conversed freely all the time (the usual signal for the coming exposure was of course given). I allowed this because, from previous observation, I had surmised that more consistent results were likely to be given when little or no reflexion was indulged in by the subject in reference to the lines and the judgments made upon them[3].

Subject C. O. K. bears out the results of the previous subject. In no case is the illusion of two eyes greater than that for one, while in the case of Table III (*b*) the illusion is greater for one eye than it is for two.

Subject J. C. shows a slight *negative* illusion for the right eye and practically no illusion for the left eye or for two eyes. It is true that the totals indicate considerably more judgments 'Vertical greater' for two eyes than for the right eye, and (to a less extent) for the left. But

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[1] My best thanks are due to Professor Herring for his kind permission to set up the apparatus in the dark room of his Physiological Laboratory, St Andrews.

[2] Even if this had been so and even if it be urged that the line first exposed, through attracting attention first, would tend to be overestimated, this would not affect the *comparative* amount of illusion for the right eye, left eye, or two eyes.

[3] Dr Rivers informs me that this is emphatically the case with experiments upon tactile discrimination. Cf. "A Human Experiment in Nerve Division," *Brain*, 1908, 354.

TABLE II. *Subject C. O. K.* (1) *Wearing glasses.*

(*a*) Standard Horizontal 50 mm.[1]

Length of Vertical	*Both Eyes* Greater	Equal	Less
48·7 mm.	23	0	1
48·1 ,,	22	0	2
47·5 ,,	20	0	4
46·9 ,,	18	0	6
46·2 ,,	11	0	13
45·6 ,,	10	0	14
45·0 ,,	3	0	21
Totals...	107	0	61

Length of Vertical	*Right Eye* Greater	Equal	Less
48·7 mm.	24	0	0
48·1 ,,	22	0	2
47·5 ,,	19	0	5
46·9 ,,	18	0	6
46·2 ,,	15	0	9
45·6 ,,	8	0	16
45·0 ,,	2	0	22
Totals...	108	0	60

Length of Vertical	*Left Eye* Greater	Equal	Less
48·7 mm.	24	0	0
48·1 ,,	24	0	0
47·5 ,,	21	0	3
46·9 ,,	17	0	7
46·2 ,,	13	0	11
45·6 ,,	8	0	16
45·0 ,,	2	0	22
Totals...	109	0	59

(*b*) Standard Vertical 47 mm.

Length of Horizontal	*Both Eyes* Greater	Equal	Less
48·1 mm.	0	0	24
48·7 ,,	1	0	23
49·4 ,,	4	0	20
50·0 ,,	7	0	18
50·6 ,,	13	0	11
51·3 ,,	17	0	7
51·9 ,,	21	0	3
Totals...	63	0	106

Length of Horizontal	*Right Eye* Greater	Equal	Less
48·1 mm.	2	0	22
48·7 ,,	4	0	20
49·4 ,,	5	0	19
50·0 ,,	8	0	17
50·6 ,,	14	0	10
51·3 ,,	15	0	9
51·9 ,,	21	0	3
Totals...	69	0	100

Length of Horizontal	*Left Eye* Greater	Equal	Less
48·1 mm.	0	0	24
48·7 ,,	0	0	24
49·4 ,,	1	0	23
50·0 ,,	4	0	20
50·6 ,,	9	0	15
51·3 ,,	15	0	9
51·9 ,,	18	0	6
Totals...	47	0	121

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[1] The lengths of the lines are given as they appear upon the screen seen by the subject. Each increment of a variable by a fraction of a millimetre as shewn, was produced by the alteration of the adjustable slide to the extent of one millimetre.

TABLE III. *Subject C. O. K.* (2) *Without glasses.*

(*a*) Standard Horizontal 50 mm.

Length of Vertical	*Both Eyes* Greater	Equal	Less
46·9 mm.	24	0	0
46·2 ,,	23	0	1
45·6 ,,	19	0	5
45·0 ,,	23	0	1
44·4 ,,	13	0	11
43·7 ,,	6	0	18
43·1 ,,	3	0	21
Totals...	111	0	57

Length of Vertical	*Right Eye* Greater	Equal	Less
46·9 mm.	22	0	2
46·2 ,,	22	0	2
45·6 ,,	22	0	2
45·0 ,,	21	0	3
44·4 ,,	14	0	10
43·7 ,,	10	0	14
43·1 ,,	5	0	19
Totals...	116	0	52

Length of Vertical	*Left Eye* Greater	Equal	Less
46·9 mm.	24	0	0
46·2 ,,	23	0	1
45·6 ,,	19	0	5
45·0 ,,	23	0	1
44·4 ,,	13	0	11
43·7 ,,	6	0	18
43·1 ,,	3	0	21
Totals...	111	0	57

(*b*) Standard Vertical 47 mm.

Length of Horizontal	*Both Eyes* Greater	Equal	Less
50·0 mm.	1	0	23
50·6 ,,	4	0	20
51·3 ,,	6	0	18
51·9 ,,	11	0	13
52·5 ,,	15	0	9
53·1 ,,	21	0	3
53·8 ,,	21	0	3
Totals...	79	0	89

Length of Horizontal	*Right Eye* Greater	Equal	Less
50·0 mm.	1	0	23
50·6 ,,	1	0	23
51·3 ,,	1	0	23
51·9 ,,	5	0	19
52·5 ,,	9	0	15
53·1 ,,	15	0	9
53·8 ,,	15	0	9
Totals...	47	0	121

Length of Horizontal	*Left Eye* Greater	Equal	Less
50·0 mm.	0	0	24
50·6 ,,	5	0	19
51·3 ,,	3	0	21
51·9 ,,	9	0	15
52·5 ,,	12	0	12
53·1 ,,	14	0	10
53·8 ,,	22	0	2
Totals...	65	0	103

Two different sets of figures had to be used according as the glasses were worn or not, as the illusion was so much greater in the latter case.

TABLE IV. *Subject J. C.*

(*a*) Standard Horizontal 60 mm.

Both Eyes

Length of Vertical	Greater	Equal	Less
61·5 mm.	29	3	0
60·75 ,,	25	2	5
60·0 ,,	16	8	8
59·25 ,,	3	9	20
58·5 ,,	1	5	26
57·75 ,,	0	4	28
Totals...	74	31	87

Right Eye

Length of Vertical	Greater	Equal	Less
61·5 mm.	20	4	8
60·75 ,,	9	10	13
60·0 ,,	6	7	19
59·25 ,,	2	8	22
58·5 ,,	0	1	31
57·75 ,,	0	1	31
Totals...	37	31	124

Left Eye

Length of Vertical	Greater	Equal	Less
61·5 mm.	26	5	1
60·75 ,,	14	12	6
60·0 ,,	9	6	17
59·25 ,,	4	4	24
58·5 ,,	0	5	27
57·75 ,,	1	3	28
Totals...	54	35	103

(*b*) Standard Vertical 60 mm.

Both Eyes

Length of Horizontal	Greater	Equal	Less
58·5 mm.	2	5	25
59·25 ,,	5	8	19
60·0 ,,	9	7	16
60·75 ,,	15	12	24
61·5 ,,	23	8	1
62·25 ,,	31	1	0
Totals...	85	41	85

Right Eye

Length of Horizontal	Greater	Equal	Less
58·5 mm.	2	10	20
59·25 ,,	8	16	8
60·0 ,,	12	11	9
60·75 ,,	24	5	3
61·5 ,,	30	2	0
62·25 ,,	32	0	0
Totals...	108	44	40

Left Eye

Length of Horizontal	Greater	Equal	Less
58·5 mm.	5	5	22
59·25 ,,	2	9	21
60·0 ,,	15	6	11
60·75 ,,	21	8	3
61·5 ,,	28	4	0
62·25 ,,	29	2	1
Totals...	100	34	58

TABLE V. *Subject A. K.*

(*a*) Standard Horizontal 60 mm.

Both Eyes

Length of Vertical	Greater	Equal	Less
54·0 mm.	32	0	0
53·25 ,,	31	0	1
52·5 ,,	19	5	8
....	....	....	....
51·75 ,,	11	3	18
51·0 ,,	10	3	19
50·25 ,,	2	4	26
Totals..	105	15	72

Right Eye

Length of Vertical	Greater	Equal	Less
54·0 mm.	28	1	3
53·25 ,,	22	4	6
52·5 ,,	16	6	10
....	....	....	....
51·75 ,,	8	5	19
51·0 ,,	5	4	23
50·25 ,,	2	3	27
Totals...	81	23	88

Left Eye

Length of Vertical	Greater	Equal	Less
54·0 mm.	29	2	1
53·25 ,,	25	3	4
52·5 ,,	19	5	8
51·75 ,,	15........	2........	15
51·0 ,,	7	2	23
50·25 ,,	2	0	30
Totals...	97	14	81

(*b*) Standard Vertical 56 mm.

Both Eyes

Length of Horizontal	Greater	Equal	Less
60·0 mm.	5	2	25
60·75 ,,	7	3	22
61·5 ,,	13........	3........	16
62·25 ,,	21	0	11
63·0 ,,	29	1	2
63·75 ,,	26	2	4
Totals...	101	11	80

Right Eye

Length of Horizontal	Greater	Equal	Less
60·0 mm.	9	6	17
60·75 ,,	12........	7........	13
61·5 ,,	21	3	8
62·25 ,,	24	2	6
63·0 ,,	28	1	3
63·75 ,,	31	0	1
Totals...	125	19	48

Left Eye

Length of Horizontal	Greater	Equal	Less
60·0 mm.	10	4	18
....	....	....	....
60·75 ,,	17	3	12
61·5 ,,	21	2	9
62·25 ,,	22	2	8
63·0 ,,	31	0	1
63·75 ,,	31	0	1
Totals...	132	11	49

this very existence of the negative illusion is obviously in contradiction with the theory of contrast with the visual field, so that this subject may be taken as contributing no evidence in favour of that theory.

With the next subject, A. K., the illusion was so great that I was unable (owing to the limitation of the apparatus) to use a standard vertical of 60 mm., as this would have required horizontals up to 68 mm. Accordingly a vertical standard of 56 mm. was used.

The results are given in Table V.

With the horizontal as standard A. K. gives about the same results for two eyes, right eye and left eye respectively; with the vertical as standard the illusion is slightly greater for two eyes than for one eye; but as the difference between the amount for two eyes and that for the right is little more than the difference between the right and left eyes taken individually, it is scarcely large enough to be of significance for us. On the whole, then, subject A. K. lends no evidence in favour of the theory in question.

Method with subject E. R. V. With subject E. R. V. I was able to carry out an exceptionally searching investigation, involving over 4300 judgments. As experiments with this subject were done when I was still without the special apparatus I prepared a series of papers on which I ruled the figures. By special care I believe I was able to measure these accurately to a very small fraction of a millimetre[1]. The papers were presented to the subject in a similar order to that used for my previous subjects, but the exposures were prolonged until the subject felt able to give her judgment. The papers were supported by a stand fixed at right angles to the field of vision about 15 inches from the subject's eyes. Large sheets were used (over 8 inches square) and the figures so arranged, towards one corner, that any effects of contrast with the margins of the sheet would be avoided. This subject was tested not only with the figure previously used, └ (Fig. A), but also with the other figures, ┘ (Fig. B), ┐ (Fig. C), ┌ (Fig. D). These were taken alternately so that practice effects might be equally distributed over all figures[2].

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[1] In one set of papers the horizontal was the standard, the vertical varying: in another set, the vertical was the standard, the horizontal varying. These sets were intermingled.

[2] Thus at one sitting the figure A would be shewn, first a series of exposures to Two eyes, then to the Right eye, then to the Left. At further sittings we took similarly B, C and D. Then the order would be changed, the figures being taken thus C, B, D, A, the order for the *eyes* in each case now being Right, Left, Two. Then again the order of figure would be D, B, A, C, the order for the eyes being Left, Two, Right—and so on.

TABLE VI. *Subject E. R. V.*

(*a*) Standard Horizontal 50 mm. Figure A ∟.

	Two Eyes		
Length of Vertical	Greater	Equal	Less
45 mm.	28	2	0
44 ,,	26	3	1
43 ,,	23	3	4
42 ,,	11	7	13
41 ,,	3	5	22
40 ,,	0	2	28
Totals...	91	22	68

	Right Eye		
Length of Vertical	Greater	Equal	Less
45 mm.	29	1	0
44 ,,	29	0	1
43 ,,	27	3	0
42 ,,	21	8	1
41 ,,	13	8	9
40 ,,	4	7	19
Totals...	123	27	30

	Left Eye		
Length of Vertical	Greater	Equal	Less
45 mm.	30	0	0
44 ,,	28	1	1
43 ,,	30	0	0
42 ,,	25	3	3
41 ,,	5	10	15
40 ,,	8	7	15
Totals...	126	21	34

(*b*) Standard Vertical 50 mm.

	Two Eyes		
Length of Horizontal	Greater	Equal	Less
56 mm.	0	0	30
57 ,,	1	2	27
58 ,,	6	4	20
59 ,,	8	4	18
60 ,,	18	2	10
61 ,,	21	0	9
Totals...	54	12	114

	Right Eye		
Length of Horizontal	Greater	Equal	Less
56 mm.	0	0	30
57 ,,	1	1	28
58 ,,	1	5	24
59 ,,	4	3	23
60 ,,	15	5	10
61 ,,	19	0	11
Totals...	40	14	126

	Left Eye		
Length of Horizontal	Greater	Equal	Less
56 mm.	0	1	29
57 ,,	0	2	28
58 ,,	1	3	26
59 ,,	0	4	26
60 ,,	7	5	18
61 ,,	19	0	11
Totals...	27	15	138

TABLE VII. *Subject E. R. V.*

(*a*) Standard Horizontal 50 mm. Figure B ⌋.

Length of Vertical	*Two Eyes* Greater	*Two Eyes* Equal	*Two Eyes* Less	Length of Vertical	*Right Eye* Greater	*Right Eye* Equal	*Right Eye* Less	Length of Vertical	*Left Eye* Greater	*Left Eye* Equal	*Left Eye* Less
45 mm.	30	0	0	45 mm.	30	0	0	45 mm.	30	0	0
44 ,,	28	2	0	44 ,,	30	0	0	44 ,,	30	0	0
43 ,,	22	2	6	43 ,,	27	1	2	43 ,,	24	5	1
42 ,,	13	5	12	42 ,,	23	4	3	42 ,,	21	3	6
41 ,,	9	5	16	41 ,,	9	6	15	41 ,,	14	5	11
40 ,,	3	2	25	40 ,,	4	3	23	40 ,,	7	2	21
Totals...	105	16	59	Totals...	123	14	43	Totals...	126	15	39

(*b*) Standard Vertical 50 mm.

Length of Horizontal	*Two Eyes* Greater	*Two Eyes* Equal	*Two Eyes* Less	Length of Horizontal	*Right Eye* Greater	*Right Eye* Equal	*Right Eye* Less	Length of Horizontal	*Left Eye* Greater	*Left Eye* Equal	*Left Eye* Less
56 mm.	1	1	28	56 mm.	0	0	30	56 mm.	0	1	29
57 ,,	1	1	28	57 ,,	0	0	30	57 ,,	0	0	30
58 ,,	0	3	27	58 ,,	1	3	26	58 ,,	0	1	29
59 ,,	6	1	23	59 ,,	4	2	24	59 ,,	1	0	29
60 ,,	18	2	10	60 ,,	17	1	12	60 ,,	9	7	14
61 ,,	18	3	9	61 ,,	21	1	8	61 ,,	13	3	14
Totals...	44	11	125	Totals...	43	7	130	Totals...	23	12	145

TABLE VIII. *Subject E. R. V.*

(*a*) Standard Horizontal 50 mm. Figure C ⏋.

Length of Vertical	*Two Eyes* Greater	*Two Eyes* Equal	*Two Eyes* Less
46 mm.	26	2	2
45 ,,	21	2	7
44 ,,	14	10	6
43 ,,	11	6	13
42 ,,	2	4	24
41 ,,	1	1	28
Totals...	75	25	80

Length of Vertical	*Right Eye* Greater	*Right Eye* Equal	*Right Eye* Less
46 mm.	25	5	0
45 ,,	22	4	4
44 ,,	19	5	6
43 ,,	12	4	14
42 ,,	3	2	25
41 ,,	2	1	27
Totals...	83	21	76

Length of Vertical	*Left Eye* Greater	*Left Eye* Equal	*Left Eye* Less
46 mm.	28	1	1
45 ,,	29	0	1
44 ,,	23	3	4
43 ,,	24	5	1
42 ,,	5	2	23
41 ,,	2	1	27
Totals...	111	12	57

(*b*) Standard Vertical 50 mm.

Length of Horizontal	*Two Eyes* Greater	*Two Eyes* Equal	*Two Eyes* Less
54 mm.	2	3	25
55 ,,	2	1	27
56 ,,	7	6	16
57 ,,	22	5	3
58 ,,	19	7	4
59 ,,	26	3	1
Totals...	78	25	76

Length of Horizontal	*Right Eye* Greater	*Right Eye* Equal	*Right Eye* Less
54 mm.	1	0	29
55 ,,	0	1	29
56 ,,	3	7	20
57 ,,	18	1	11
58 ,,	17	2	11
59 ,,	24	2	4
Totals...	63	13	104

Length of Horizontal	*Left Eye* Greater	*Left Eye* Equal	*Left Eye* Less
54 mm.	0	0	30
55 ,,	1	0	29
56 ,,	0	2	28
57 ,,	11	4	15
58 ,,	13	2	15
59 ,,	16	5	9
Totals...	41	13	126

TABLE IX. *Subject E. R. V.*

(*a*) Standard Horizontal 50 mm. Figure D ┌.

Length of Vertical	*Two Eyes* Greater	Equal	Less
46 mm.	23	4	3
45 „	13	6	10
44 „	6	2	22
43 „	2	4	24
42 „	1	1	28
41 „	0	0	30
Totals...	45	17	117

Length of Vertical	*Right Eye* Greater	Equal	Less
46 mm.	27	2	1
45 „	20	5	4
44 „	8	8	15
43 „	3	5	22
42 „	1	0	29
41 „	0	1	29
Totals...	59	21	100

Length of Vertical	*Left Eye* Greater	Equal	Less
46 mm.	29	1	0
45 „	28	2	0
44 „	14	11	5
43 „	7	11	12
42 „	0	3	27
41 „	0	0	30
Totals...	78	28	74

(*b*) Standard Vertical 50 mm.

Length of Horizontal	*Two Eyes* Greater	Equal	Less
54 mm.	3	0	27
55 „	11	3	16
56 „	13	3	14
57 „	23	6	1
58 „	29	0	1
59 „	28	0	2
Totals...	107	12	61

Length of Horizontal	*Right Eye* Greater	Equal	Less
54 mm.	1	2	27
55 „	9	7	14
56 „	12	8	10
57 „	25	2	3
58 „	27	3	0
59 „	28	1	1
Totals...	102	23	55

Length of Horizontal	*Left Eye* Greater	Equal	Less
54 mm.	0	0	30
55 „	1	1	28
56 „	2	5	23
57 „	17	0	13
58 „	17	3	10
59 „	23	4	3
Totals...	60	13	107

Results of subject E. R. V. This subject gives striking results. In no case is the illusion greater for two eyes than it is for one eye, either the right or the left. Indeed for every figure, the illusion is always greater for the left eye alone (and sometimes greater for the right eye alone) than it is for two eyes.

All these five subjects, then, who were tested by the method of right or wrong cases shew evidence which is overwhelmingly against the theory that explains the illusion by reference to the oval shape of the field of vision.

III. *Experiments by Method of Mean Error.* Similar results are shewn by the three subjects tested by the Method of Production (Mean Error). With these subjects I used standard lines of three different lengths, 40 mm., 60 mm. and 100 mm. Each standard line was ruled by myself on a separate square sheet of paper. The subject drew three with a standard of 40 mm., three of 60 mm., then three of 100 mm. Then the order was reversed, and so on, so that any effects of practice or contrast might affect all alike. I was desirous of finding out whether the illusion increased proportionately with the length of the lines. As will be seen this was by no means the case[1]. Each sitting lasted about half an hour and was divided into two parts. At one sitting the right eye was used first and then the left; at the next the left was used, and then two eyes; at the next two eyes first, followed by the right. Then for the next three sittings the order would be completely reversed. Subject A. D. drew twenty lines for each standard size with each eye, ten with vertical as standard, and ten with horizontal as standard. The other two subjects drew twelve lines for each standard size. Thus the results of A. D. are based upon one hundred and eighty drawings and judgments, those of A. S. and L. B. upon one hundred and eight.

The subjects were allowed to take their time in drawing, and were allowed to rub out or lengthen lines until satisfied. They were strictly enjoined to hold the paper at arm's length, vertically, when judging, and right opposite to the eye in use. As I took each subject individually I was able to see that these instructions were carried out with care.

The results are given in Table X, the standard variations σ being added in brackets.

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[1] In the cases of two subjects there is a suggestion that a maximum amount of illusion is reached with a given length of the lines after which further increase of the lines leads to a *decrease* in the amount of the illusion.

TABLE X.

Subject A. D.

	Standard Horizontal 40 mm.		Standard Vertical 40 mm.	
Two Eyes...	Vertical	39·9 mm. (0·83 mm.)	Horizontal	42·75 mm. (0·84 mm.)
Right Eye...	,,	40·2 mm. (1·17 mm.)	,,	43·3 mm. (1·16 mm.)
Left Eye ...	,,	40·2 mm. (1·62 mm.)	,,	43·6 mm. (0·92 mm.)
	Standard Horizontal 60 mm.		Standard Vertical 60 mm.	
Two Eyes...	Vertical	58·5 mm. (0·77 mm.)	Horizontal	63·5 mm. (1·09 mm.)
Right Eye...	,,	58·55 mm. (1·66 mm.)	,,	62·65 mm. (1·09 mm.)
Left Eye ...	,,	58·3 mm. (1·65 mm.)	,,	64·3 mm. (1·15 mm.)
	Standard Horizontal 100 mm.		Standard Vertical 100 mm.	
Two Eyes...	Vertical	98·85 mm. (2·43 mm.)	Horizontal	101·85 mm. (2·62 mm.)
Right Eye...	,,	100·25 mm. (2·72 mm.)	,,	102·35 mm. (3·85 mm.)
Left Eye ...	,,	99·85 mm. (4·22 mm.)	,,	103·6 mm. (2·33 mm.)

Subject L. B.

	Standard Horizontal 40 mm.		Standard Vertical 40 mm.	
Two Eyes...	Vertical	36·25 mm. (0·85 mm.)	Horizontal	43·3̇ mm. (1·24 mm.)
Right Eye...	,,	36·5 mm. (1·44 mm.)	,,	44·91 mm. (1·3 mm.)
Left Eye ...	,,	35·83 mm. (0·42 mm.)	,,	44·37 mm. (1·48 mm.)
	Standard Horizontal 60 mm.		Standard Vertical 60 mm.	
Two Eyes...	Vertical	54·63 mm. (2·05 mm.)	Horizontal	63·83 mm. (2·11 mm.)
Right Eye...	,,	53·0 mm. (1·52 mm.)	,,	64·5 mm. (1·7 mm.)
Left Eye ...	,,	54·0 mm. (1·44 mm.)	,,	64·58 mm. (1·55 mm.)
	Standard Horizontal 100 mm.		Standard Vertical 100 mm.	
Two Eyes...	Vertical	93·58 mm. (1·36 mm.)	Horizontal	102·75 mm. (1·22 mm.)
Right Eye...	,,	95·3̇ mm. (3·34 mm.)	,,	104·0 mm. (0·8 mm.)
Left Eye ...	,,	95·83 mm. (2·98 mm.)	,,	102·75 mm. (1·87 mm.)

Subject A. S.

	Standard Horizontal 40 mm.		Standard Vertical 40 mm.	
Two Eyes...	Vertical	38·75 mm. (0·65 mm.)	Horizontal	44·0 mm. (1·5 mm.)
Right Eye...	,,	38·6 mm. (2·28 mm.)	,,	44·3̇ mm. (1·9 mm.)
Left Eye ...	,,	37·6 mm. (2·49 mm.)	,,	42·83 mm. (1·37 mm.)
	Standard Horizontal 60 mm.		Standard Vertical 60 mm.	
Two Eyes...	Vertical	55·3 mm. (2·05 mm.)	Horizontal	64·17 mm. (1·39 mm.)
Right Eye...	,,	54·83̇ mm. (2·73 mm.)	,,	64·5 mm. (2·29 mm.)
Left Eye ...	,,	54·0 mm. (1·82 mm.)	,,	61·83 mm. (1·86 mm.)
	Standard Horizontal 100 mm.		Standard Vertical 100 mm.	
Two Eyes...	Vertical	92·6 mm. (2·31 mm.)	Horizontal	107·0 mm. (2·45 mm.)
Right Eye...	,,	91·83̇ mm. (6·98 mm.)	,,	106·8 mm. (5·79 mm.)
Left Eye ...	,,	92·33 mm. (1·96 mm.)	,,	101·6̇ mm. (1·89 mm.)

Combining the results given by both horizontal and vertical standards we get the following figures showing the average illusion for each standard.

TABLE XI.

		Amount of Illusion		
		Two Eyes	*Right Eye*	*Left Eye*
Subject A. D.......	Standard 40 mm.	1·42 mm.	1·55 mm.	1·7 mm.
,, ...	,, 60 ,,	2·5 ,,	2·05 ,,	3·0 ,,
,, ...	,, 100 ,,	1·5 ,,	1·05 ,,	1·87 ,,
Subject L. B.......	,, 40 ,,	3·54 ,,	4·2 ,,	4·9 ,,
,, ...	,, 60 ,,	4·6 ,,	5·75 ,,	5·29 ,,
,, ...	,, 100 ,,	4·66 ,,	4·25 ,,	3·42 ,,
Subject A. S.......	,, 40 ,,	2·62 ,,	2·85 ,,	2·61 ,,
,, ...	,, 60 ,,	4·41 ,,	4·83 ,,	3·91 ,,
,, ...	,, 100 ,,	6·95 ,,	7·27 ,,	4·65 ,,
Average for all Standards...		3·57 mm.	3·75 mm.	3·48 mm.

It will be noticed that in eight cases out of nine the illusion for two eyes is actually either less than that for the right eye or less than that for the left. In only one case (L. B. standard 100 mm.) is it greater for two eyes than for both the left and the right eye: and this subject more than counterbalances this by showing for the other two standards a greater illusion for each eye independently than for two eyes.

The averages for all subjects and all standards, based upon nearly 400 drawings, show that the illusion for two eyes is slightly less than that for the right eye and slightly greater than that for the left.

IV. *Conclusion from both methods.* Thus the 'method of production' confirms the results gained by the more exact 'method of right and wrong cases.' There is no evidence that the illusion for two eyes tends to be greater than that for one alone. On the contrary there is sometimes a tendency for it to be a mean between the amounts of illusion for each eye taken separately: and sometimes the amount of illusion for two eyes is actually less than the amount for either the right or the left eye.

C. LIPPS'S AESTHETIC THEORY OF THE ILLUSION.

I. *The doctrine of aesthetic Einfühlung applied to geometrical figures.* According to Lipps, experience has made it impossible for us to see a line without feeling or thinking into it a movement, a kind of "active power[1]." This aesthetic *Einfühlung* (Empathy, as Dr Ward translates it) is bound up with our very mode of apperceiving lines. According to Lipps, as no way of viewing a thing is so unintentional as the aesthetic, so no way is so unavoidable. The vertical line seems to strive upwards. The curve bends itself. The 'standing' rectangle ▯ holds itself in and so gets power to stand up. The 'lying' rectangle ▭ stretches itself out or "lets itself go." The circle presses towards its centre, overcoming the tendency to tangential motion. It would be strange, argues Lipps, if this ever-present suggestion did not affect our judgment and cause optical illusions[2]. The square, for example, appears to us as a figure which raises itself up more than it spreads itself out. For in the vertical direction it has to overcome the force of gravity, whilst in the horizontal direction there is only its own cohesive force to be overcome by the tendency to stretch itself out. The greater the power which is overcome, the greater, presumably, is the power which overcomes. Similarly the simple vertical line will appear longer than the horizontal.

II. *Experimental Investigation of Lipps's Theory.* It is obviously difficult to apply experimental methods to the test of this theory. Introspective testimony as to the *absence* of any such *Einfühlung* cannot be taken as final. But it happens that Lipps describes conditions under which this aesthetic cause of optical illusions may be emphasised. He gives the following figure (Fig. 1) as an example[3].

The square in Fig. 1 is the same size as that in Fig. 1 *a*, but it appears taller. The figure as a whole "raises itself up" and the enclosed square shares in this. The weight of the figures above apparently emphasise the activity of the vertical lines of the square in this resistance to gravity.

Here, then, we have a case where the normal over-estimation of the vertical is still further emphasised, and hence arises a possible opening for experiment.

III. *Method of Experiment.* I determined to set these figures before a large number of subjects, asking whether the enclosed square in Fig. 1 seemed to them the same height as Fig. 1 *a*, or whether either appeared the bigger, either in height or width. It seemed legitimate to argue thus: if subjects over-estimate vertical lines because of the peculiar *Einfühlung* of which Lipps writes, then they are likely to show this aesthetic effect in the figure, where, Lipps asserts, the aesthetic *Einfühlung* is *emphasised*. Whereas, if there are any who do not experience the emphasised form of the illusion, they are not likely to show any illusion in the ordinary figure of the vertical and horizontal lines, *if* this simpler form of the illusion is due to the aesthetic *Einfühlung*, as Lipps maintains. Or to put it otherwise: if we divide the subjects into two groups according to whether they show the Lippsian effect in Fig. 1 or not, then if those who show that effect also show the positive illusion when comparing a simple vertical line with an adjoining horizontal, whereas those who do not show the Lippsian effect in Fig. 1 also show no illusion with the ordinary horizontal-vertical lines, there would be experimental evidence in favour of this aesthetic theory of the illusion. Whereas if one found no correlation between the two illusions, there would be evidence against this theory of Lipps. This latter proved to be the case.

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[1] Compare the way in which Hume traces our ideas of natural causes and forces to our own subjective experience. Cf. Prof. Stout's *Analytic Psychology*, I. 177–9.

[2] See Article, "Aesthetische Faktoren der Raumanschauung" in *Beitr. zur Psychol. und Physiol. der Sinnesorg.*, published as *Festgruss* to Helmholtz, Leipzig, 1891.

[3] *Op. cit.*, 224.

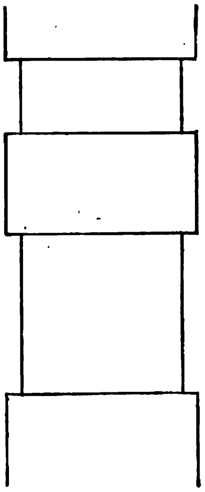
FIG. 1.

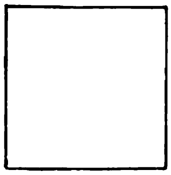
FIG. 1 *a*.

I performed the tests upon about eighty subjects. Not one of them was aware of the nature of the illusion which was being tested. I used the actual figures in Lipps's own essay, of which Figs. 1 and 1 *a* are copies. The subjects took the book in their hands, holding the page at right angles to the line of vision, and then recorded their judgment in writing or communicated it to me verbally. Care was taken to avoid the slightest suggestion in the putting of the question.

In reference to the illusion of Fig. 1, I found that a surprisingly large proportion of subjects flatly contradict Lipps's statement, some 20% actually asserting the opposite, viz. that the separate square appeared higher, while over 20% said that the squares appeared equal in height[1].

The tests were performed upon groups of students in training at St Andrews University and at the Training College, Dundee. The results are shown in Tables XII, XIII and XIV.

In Groups 1, 2 and 3 for the estimation of the amount of illusion in the simple horizontal-vertical line figure, the same method of Mean Error was used as that described earlier in this paper. The standard lines ruled in this case were all 60 mm. During the drawing of the lines I was careful to see that all subjects held the papers at arm's length and at right angles to the line of vision for their final judgment. They were also told (at least in the case of Groups 1 and 2) to fixate the point of junction of the lines in making their final decision. None of the lines was measured by the students, so that none of them knew how they were progressing until all the experiments were over. In Group 3 all four figures were used:

└ ┘ ┐ ┌,

which I call A, B, C and D respectively. In Groups 1 and 2 only figures B and C were used, 10 lines being drawn for each figure, 5 with the horizontal as standard and 5 with the vertical as standard. In Group 3 only 4 lines were drawn in each figure, this being really the first preliminary test. These numbers may appear small: but they amount to a total of 20 lines for each subject in Groups 1 and 2 and to 16 lines each in Group 3—quite adequate, I think, when a large number of subjects are being used[2].

---

[1] Lipps also states that the square in Fig. 1 appears narrower than Fig. 1*a*. This is contradicted even more frequently by my subjects. But I have divided them according to their judgments of the height, as this is the primary thing. All possible combinations of judgments were given, a few even thinking Fig. 1*a* both taller and wider than the square in Fig. 1.

[2] Mr W. H. Winch, for example, was able to show a gradual decrease of this illusion with increasing age in school children on the basis of only three lines drawn in each of his three figures by each subject. See this *Journal*, II. 220.

TABLE XII.

GROUP 1. Women Students.

Average amount of Illusion:	Fig. B.	Fig. C.
For fourteen subjects who show the Lippsian illusion in Figs. 1 and 1 *a*	2·2 mm., Av. m.v. 1·11 mm.[1]	2·4 mm., Av. m.v. 1·0 mm.
For five subjects who do *not* show Lippsian illusion in Figs. 1 and 1 *a*	3·06 mm., Av. m.v. 1·36 mm.	2·12 mm., Av. m.v. 1·26 mm.

TABLE XIII.

GROUP 2. Men Students.

Average amount of Illusion:	Fig. B.	Fig. C.
For eleven subjects who show the Lippsian illusion in Figs. 1 and 1 *a*	0·87 mm., Av. m.v. 1·2 mm.	2·14 mm., Av. m.v. 1·1 mm.
For five subjects who do *not* show Lippsian illusion in Figs. 1 and 1 *a*	1·12 mm., Av. m.v. 1·0 mm.	1·93 mm., Av. m.v. 0·94 mm.

TABLE XIV.

GROUP 3. Women Students.

Average amount of Illusion:	Fig. A.	Fig. B.	Fig. C.	Fig. D.
For eight subjects showing Lippsian illusion in Figs. 1 and 1 *a*	2·12 mm., Av. m.v. 1·06 mm.	1·71 mm., Av. m.v. 0·74 mm.	2·65 mm., Av. m.v. 0·87 mm.	1·96 mm., Av. m.v. 0·88 mm.
For seven subjects *not* showing Lippsian illusion in Figs. 1 and 1 *a*	2·92 mm., Av. m.v. 1·34 mm.	2·06 mm., Av. m.v. 1·13 mm.	2·05 mm., Av. m.v. 1·17 mm.	3·81 mm., Av. m.v. 0·98 mm.

If, as we said, the ordinary over-estimation of the vertical is due entirely to some such *Einfühlung* as Lipps describes, then the 'non-Lippsians' ought to show no illusion. If the *Einfühlung* were only a co-operating factor they ought at least to show, in the long run, less illusion in the ordinary horizontal-vertical figure than those who have the illusion in Fig. 1. Now it will be seen that no such results are found. In Group 1, the average illusion for the 'non-Lippsians' is actually greater than that of the 'Lippsians' for Fig. B, while for Fig. C it is practically the same for both sections. In Group 3 again the 'non-Lippsians' show the greater illusion in three out of the four figures.

In Group 2 the 'Lippsians' show a slightly greater illusion for Fig. C, the 'non-Lippsians' for Fig. B.

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[1] The m.v. is not given. For the argument is that Lipps's aesthetic illusion has no connection with the horizontal-vertical illusion. Hence subjects grouped according to their agreement or disagreement with Lipps, might differ widely in the amount of hor.-vert. illusion they showed. But a large m.v. might give an erroneous impression of unreliability at first sight.

IV. *Supplementary Experiments.* I had already performed experiments upon two other small groups of students numbering twenty-seven in all, by means of the simple apparatus devised by Dr Rivers, consisting of a small blackboard, with adjustable white strings at right angles to one another[1]. Here again all four figures, A, B, C and D were used in the testing. Ten attempts were made to make the vertical equal to the standard horizontal (60 mm.) for each figure, so that forty judgments were given by each subject. Unfortunately I had not time to give a series with the vertical as standard. Consequently the influence of the tendency to make the variable long, which Dr Rivers has demonstrated[2], resulted in much smaller amounts of illusion. But this does not affect the *comparison* of the results.

These subjects also expressed their judgments upon Fig. 1 and Fig. 1 *a*. If we divide them according to their agreement with Lipps in reference to these two figures we get the following results[3].

In the first group, 4 'Lippsians' show an average negative illusion of −0·69 mm. in the ordinary horizontal-vertical figure.
7 'non-Lippsians' show an average positive illusion of 1·55 mm.
In the second group, 8 'Lippsians' show an average positive illusion of 0·68 mm.
8 'non-Lippsians' show an average negative illusion of −1·39 mm.

V. We see that of all the five groups tested, only this last group of subjects offers any support for the Lippsian theory, assuming the validity of the argument that, if the aesthetic *Einfühlung* is at work in the ordinary illusion, it is also at work in what Lipps himself regards as an emphasised form of the illusion, and *vice versa*. Taking the five groups altogether, the evidence is overwhelmingly against the theory. On the average those who show the greatest illusion in the case of simple horizontal-vertical lines are those who do *not* reveal the influence of the aesthetic *Einfühlung* in their judgments of Fig. 1 and Fig. 1 *a*[1].

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[1] See Rivers, "Observations on the Senses of the Todas," this *Journal*, I. 340.

[2] *Ibid.* 349.

[3] It is worthy of note that the mean variations were, on the whole, considerably smaller in the case of these experiments with the blackboards than they were when the lines had to be drawn on paper. Frequently the m.v. was less than 0·5 mm. for a variable of 60 mm. (10 judgments).

VI. *Aesthetic Einfühlung in school children.* In some experiments upon the horizontal-vertical illusion in school children, Mr W. H. Winch[2] found that the illusion decreased fairly steadily, on the average, from the age of 8 to that of 14. Accordingly I decided to see if Lipps's aesthetic illusion of Figs. 1 and 1 *a* occurs less frequently among children as the age increases. I performed the tests upon seventy-one children, varying in age from 8 to 14[3]. Each child was taken individually and great care was taken to avoid any suggestion in the putting of the question. I used the actual figures in Lipps's article. Practically the same words were used in giving instructions to all the children. The squares were *pointed* out, and not *called* 'squares' but 'figures'; and the children were asked "Does this figure [pointing to it] seem to you the same height as this, or does one appear taller than the other?" The results are given in Table XV.

TABLE XV.

Judgments of school children as to Fig. 1 and Fig. 1 a.

Age	Number of subjects	Number of judgments as to height of Fig. 1 *a*, compared with that of square in Fig. 1		
		Less	Equal	Greater
Between 13 and 15 years	14	12	1	1
,, 12 ,, 13 ,,	14	11	3	0
,, 11 ,, 12 ,,	12	7	4	1
,, 10 ,, 11 ,,	12	9	3	0
,, 9 ,, 10 ,,	11	10	1	0
,, 8 ,, 9 ,,	8	5	1	2
	71	54	13	4

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[1] It is doubtless true that the illusion of Fig. 1 depends partly upon the degree to which the attention takes in the whole figure and is not merely concentrated upon the enclosed square. But it scarcely seems likely that many subjects will escape entirely the *suggestion* of the structure over the square, with its consequent effects. For according to Lipps we are concerned with one of the most subtle and unavoidable things. Furthermore there would remain the cases of those subjects to whom the square enclosed in Fig. 1 actually looks lower than that in Fig. 1*a* and who nevertheless show a marked illusion in the ordinary horizontal-vertical figure.

[2] This *Journal*, II. 220.

[3] My best thanks are due to Mr J. Williamson, Headmaster of South Tay Street School, Dundee, for his kind permission to visit this school for the purpose of these experiments.

Now it will be seen that the proportion of those who show the Lippsian illusion, *i.e.* who judge the height of Fig. 1 *a* to be less than that of Fig. 1, does not decrease as the age of the group increases. Unfortunately I was not able to test these same children with the ordinary horizontal-vertical figure. But as Mr Winch based his conclusion upon 165 children from three different schools it may be fairly assumed that the decrease with age in the ordinary illusion is general among school children.

Now if the ordinary horizontal-vertical illusion is due to aesthetic *Einfühlung* it seems strange that corresponding to the decrease in the number of children showing a marked illusion (and increase in the number of children showing *no* illusion), there is no decrease in the number of children showing the same type of illusion in the form in which it appears in Fig. 1.

Of course one cannot press this kind of argument too far: practice in drawing the square may be the source of the decrease with age of the illusion in the ordinary horizontal-vertical figure[1], whilst this might not show its effect in the judgment of Fig. 1, unless the attention was concentrated upon the square alone. Further it will be noticed that the proportion of children who show the Lippsian effect in Fig. 1 (54 out of 71) is considerably greater than the proportion of adults who do the same (45 out of 77) and children in general show the ordinary horizontal-vertical illusion to a greater extent than do adults. So perhaps the most one can say is that the experiments upon the children show at least no unambiguous evidence in favour of the theory of Lipps.

VII. *The 'Standing' and the 'Hanging' Vertical.* One further possible method occurred to me for putting to the test the theory of Lipps. In the figure B ┐ the vertical probably appears to most people as hanging from the horizontal, whilst in figure A ┘ it appears to stand[2]. I confirmed this by testing individually a number of my subjects (of Tables XII and XIII). Of thirty-seven, only two regarded the vertical as standing in Fig. B, thirty-four at once stated that the vertical appeared to hang, one saying that it did neither, but simply 'lay on the paper.' Now from the language of Lipps's article in the Helmholtz *Festgruss* one might infer, if his theory be right, that the hanging vertical, not showing the "liveliness" of that which raises itself against gravity, should not be over-estimated, as is the standing vertical, when compared with the horizontal. But my results show that the illusion is even greater in the case of the hanging vertical than for the standing. The average illusion for all subjects in Group 1 is 1·23 mm. for the 'standing,' and 2·72 mm. for the 'hanging' vertical; for Group 2, for 'standing' vertical 2·6 mm., and for 'hanging' vertical 2·82 mm.; similarly for Group 3, the average of Figs. A and B (vertical 'standing') is 2·18 mm., whilst that for Figs. C and D (vertical 'hanging') is 2·61 mm.[1]

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[1] Mr Winch suggests that training in drawing the square may explain why the illusion in the case of children is less for the figure ┘ than for the figure +. But if this were the case one would expect the *difference* between the amounts of illusion for these figures to increase as the age of the children increased. Whereas if the average of the oldest three groups of children (*op. cit.* p. 222) be compared with that of the youngest three groups, it will be found that the opposite is the case.

[2] In the small figures as printed this may not be the case. But the test should be tried with figures ruled in the centre of large squares of paper, and with subjects who are ignorant of any theories upon the matter.

A more subtle discussion in the later *Raumaesthetik und geometrisch-optische Täuschungen* may possibly be held to provide a way out of the difficulty for Lipps. He there maintains[2] that there are two tendencies at work in the vertical line, the stretching upwards against gravity, and the tendency to hold itself in, a kind of cohesion. In all lines there are two tendencies in evidence: (*a*) the primary activity without which the line would not exist—in the case of the vertical this is the activity upwards against gravity, without which no line would raise itself at all, but would vanish in the point at the lower end; and (*b*) the secondary tendency[3] which *limits* that primary activity, without which the primary activity would, so to speak, carry the vertical line upwards to infinity. Now it is always in the direction of the *primary* activity that the illusion of over-estimation takes place. Hence the 'standing' vertical is over-estimated because its primary activity is upwards, lengthening the line. But what of the 'hanging' vertical? Here, says Lipps, the primary activity is the downward pull of gravity: this is the victorious force without which the line would contract into nothing. And as it is always in the direction of the victorious primary activity that the illusion occurs, the hanging vertical also is over-estimated. One can only reply to such an argument in a similar vein.

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[1] In averaging, negative illusions are subtracted from the total of positive illusions.

[2] See pp. 62, 64, 104 and 108.

[3] Lipps prefers to keep the term 'activity' for the primary tendency, as being essentially the active one, whilst to the secondary he would only apply the term 'tendency.' This secondary tendency must wait for the primary to work before it can come into existence, and then only as a negative, hindering action.

One might argue as follows. In the case of the 'hanging' vertical the force of gravity is victorious. But in the 'standing' vertical we have an upward force even greater than gravity, for the line stretches itself up against it. Then seeing that this upward force is so much greater than gravity it ought to cause a greater illusion in the case of the 'standing' vertical than it does in the 'hanging.' Such an argument may seem only so much 'hair-splitting,' but it is analogous to that on which Lipps's theory of the illusion depends. For in contrast with the vertical the horizontal must be supposed to embody a lesser degree of stretching activity, only sufficient to overcome the contraction inwards.

VIII. *Conclusion as to Lipps's theory.* Without then disparaging the value of much of Lipps's work in the field of Mechanical Aesthetics, one may say that his theories scarcely form a satisfactory explanation of the horizontal-vertical illusion, and that in so far as experiments can be used to test them, the evidence goes entirely against their validity, though in the nature of the case it is scarcely possible to provide an absolute experimental disproof.

One reservation should be added. While we may regard Lipps's theory as quite inadequate to explain the illusion as it occurs with the ordinary figure used in these experiments, yet it remains possible that there may be some additional emphasis and over-estimation of the vertical lines in complex figures which produce an aesthetic attitude on the part of the observer, as for example in architecture. But the important fact for us is that the illusion in question persists even where the attitude is critical rather than aesthetic.

D. SUMMARY OF RESULTS AND CONCLUSIONS.

I. The theory that the illusion is caused by contrast with the visual field is disproved by the fact that the illusion is not less for one eye taken alone than for two eyes together.

II. The amount of the illusion for two eyes is sometimes a mean between the amounts for each eye taken individually: or it may even be less than that for either eye.

III. The difference in some subjects between the amount of illusion for the right eye and that for the left suggests the influence of a physiological factor.

IV. Increase in the size of the lines does not necessarily involve an increase in the amount of the illusion. In some cases a maximum amount of illusion seems to be reached with a given length of the lines, after which a further increase of the lines leads to a decrease in the amount of the illusion.

V. Lipps's theory of the illusion seems incapable of direct disproof by means of experiment. But there is an entire absence of correlation (which should be shown if the theory were correct) between the results of the tests upon the horizontal-vertical illusion and those of an illusion having, according to Lipps, a similar aesthetic cause.

VI. The illusion in the case of the 'hanging' vertical is on the average appreciably greater than that in the case of the 'standing' vertical. This is contrary to what one would expect if Lipps's theory were correct.

VII. Lipps's 'aesthetic' illusion of Fig. 1 is shown by a greater proportion of school children than of adults. But the proportion does not decrease in children from eight years up to fourteen years of age, as does the ordinary illusion of the 'horizontal-vertical' figure.

VIII. In at least three subjects, the effect of practice was most unusual, viz.: the amount of the illusion was *increased* by practice. This point will be further referred to in a forthcoming paper, where also evidence in favour of a physiological theory of the illusion will be submitted, and the effect of astigmatism upon the illusion will be discussed.

I have already expressed my deep indebtedness to Dr Rivers. It remains for me to tender my thanks to those who so kindly submitted to somewhat long and tedious experiments—Miss C. O. Kerr, and Messrs W. Appleton, L. Brehaut, A. Donald, A. Kydd, A. Stout and J. Chalmers, and to my wife both for acting as a subject and for valuable assistance with the mass of minor arithmetical calculations.

(*Manuscript received August 10th,* 1911.)

THE ILLUSION OF FILLED AND UNFILLED SPACE.

BY E. O. LEWIS[1].

(From the Cambridge Psychological Laboratory.)

I. Explanations of the illusion—Helmholtz, Hering, Wundt, Lipps.
II. Apparatus and method of Experiment; Hales's tachistoscope.
III. Problems investigated, and the results obtained:
  (a) The illusion decidedly greater with momentary than prolonged exposures.
  (b) The apparent exception of bisected space.
  (c) Effects of practice upon the illusion; disappearance of the illusion with practice at prolonged observation; its persistence practically unchanged with practice at momentary observation.
  (d) Variation of the illusion with the number of divisions in the filled space; maximal illusion when there are about eight divisions.
  (e) Symmetrical and unsymmetrical figures; considerable decrease in the illusion with prolonged observation when the figure becomes unsymmetrical; but no marked decrease with momentary observation.
IV. Summary.

I. EXPLANATIONS HITHERTO ADVANCED.

ONE of the earliest explanations of the illusion of filled and unfilled space (Fig. 1) was that put forward by Helmholtz[2]. He attributed the illusion to contrast effects; the length of the whole distance *AB* forming such a contrast with the length of each of the subdivisions that the whole is overestimated as compared with the undivided space *BC* and the subdivisions themselves are underestimated when judged independently. Hering's[1] explanation of this illusion is better known. He assumed that our judgment of the distance between two points is conditioned by the length of the retinal chord between the points stimulated. The sum of the retinal chords joining the points stimulated on the retina by the dots which divide *AB*, is obviously greater than the length of the one chord joining the points of stimulation corresponding to *BC*; and this, according to Hering, explains the overestimation of *AB* as compared with *BC*. Wundt[2] attributes the illusion to the impeded movement and the greater effort necessary when the eye moves along the filled space, as compared with the free and easy movement when the unfilled space is observed. Lipps[3], in accordance with his general theory of geometrical-optical illusions, refers the illusion to the summation of the expansive tendencies of the independently apprehended parts.

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[1] Dr R. R. Rusk collaborated with me in the earlier part of this investigation, and gave me considerable help in writing this article. I am also indebted to Dr C. S. Myers for several valuable suggestions.

[2] *Handbuch d. physiol. Optik*, 2te Aufl., 703.

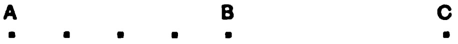

FIG. 1.

II. APPARATUS AND METHOD OF EXPERIMENT.

This investigation is a continuation of the work done on geometrical-optical illusions in the Cambridge Psychological Laboratory during recent years[4]. The apparatus used, Hales's tachistoscope, has been already described in a previous article[5]. The only part of the apparatus which requires to be described here, is the special plate used for presenting the illusion. The brass slide *Y* (Fig. 2) contained small holes arranged so as to form a figure similar to Fig. 1. On this slide there were two such figures, namely, *ABC*, in which the standard *AB* was 50 mm. long, and *abc*, in which the standard *ab* was 30 mm. long. When the slide was against the knob *N*, as in Fig. 2, the figure *abc* was in a position corresponding to an aperture in the plate *XX*, and thus the light passed through the holes, and an image of this figure was thrown on the screen of the apparatus. In this position of the slide no image of *ABC* was formed, because the plate *XX* contained no apertures corresponding to the holes of this figure. In order to get an image of *ABC*, the slide was moved against the knob *M*, which brought *ABC* into the original position of *abc*. Whereas the filled spaces *AB* and *ab* were of constant length, the unfilled spaces *BC* and *bc* were variable, *C* and *c* being on movable slides. With this plate, readings with two standards could be easily obtained. A few readings were taken with one standard, and then a few with the other, the subject not being informed as to which standard was being used. This arrangement prevented the subject acquiring an absolute impression of either standard which would have resulted in a tendency to attend to the variable only.

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[1] Myers, *Text-book of Exper. Psychol.*, 2nd ed., 1911, I. 284.

[2] *Abh. d. kön. säch. Gesell. d. Wiss*, XXIV. 55.

[3] *Raumästhetik und geom.-optische Täuschungen*, Leipzig, 1897, 141–4.

[4] This *Journal*, 1908, II. 243, 294; 1909, III. 21.

[5] *Ibid.* 1908, II. 244 ff.

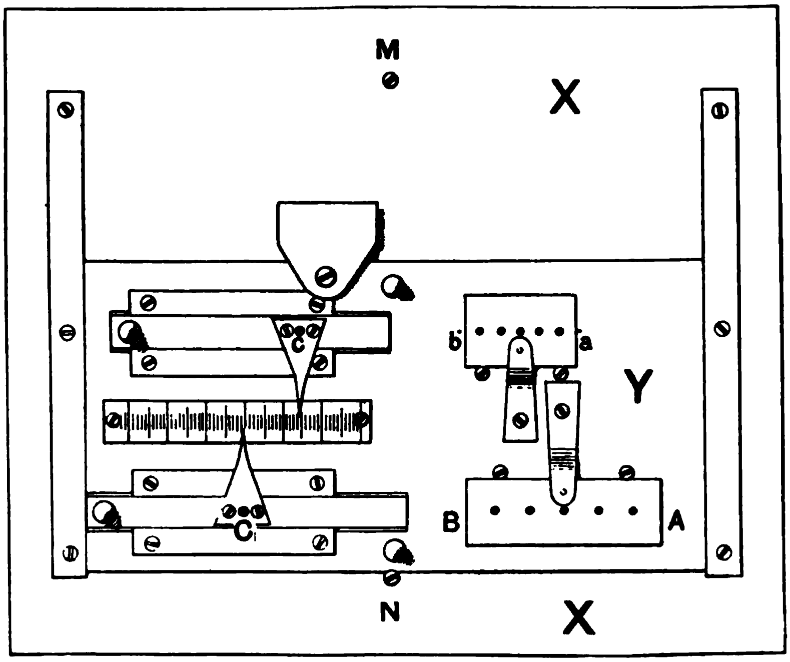

Fig. 2.

The method of obtaining readings has also been described at length[1]; it was a modification of the method of right and wrong cases. The smallest variation of the variable in these experiments was ·5 mm. and the greatest 3 mm. A typical record of the readings is given below[1], which exemplifies the definiteness of the results obtained. As in previous investigations, the judgments with the momentary exposures were more consistent and were given with less hesitation than those with the prolonged exposures. Five readings were generally made with each length of the variable for prolonged and momentary exposures; and as there were two standards, about one hundred readings were taken at each sitting. The amount of the illusion was in most cases decided by the results of two such sittings. A greater number of readings would have been taken were it not that practice in the case of prolonged exposures modified the amount of the illusion considerably[2]. The order of the prolonged and momentary readings was varied at different sittings. Some subjects gave readings with figures in which the standards were on the left; and others with figures with the standards on the right. A few subjects made observations with the standards in both positions, but in none of them did the amount of the illusion vary much with the position of the standard.

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[1] *Op. cit.*

III. PROBLEMS INVESTIGATED AND THE RESULTS OBTAINED.

(*a*) *The amount of the illusion with prolonged and momentary exposures.* Readings were obtained with figures similar to Fig. 1, the filled space being divided into four equal divisions. Readings were given by six adults and three boys (ages 13—15). The results of each individual's readings are given in Table I. P and M denote prolonged and momentary readings respectively. L and R indicate whether the standard was leftward or rightward of the variable. m.v. devotes the mean variation.

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[1] The following is a record of readings obtained from one of the subjects. It is seen that the amount of the illusion is determined even to the nearest half-millimetre. S = distinctly shorter, s = shorter, s' = slightly shorter, L = distinctly longer, l = longer, l' = slightly longer, n = no difference.

Standard 30 mm.

Prolonged Readings		Momentary Readings	
mm.		mm.	
34	*S s S S s s s s′ S s*	35·5	*s S s S S S s S s s*
34·5	*s S s′ n s n s l′ s′ l′*	36	*s′ s s s S S s s n l′*
35	*s′ l′ l l′ n s′ n l′ n s′*	36·5	*l′ l n s′ l′ s′ s′ l n l′*
35·5	*l l′ L l′ n l l′ l′ l l*	37	*l L l′ l L l l l′ l l*
36	*l l l′ L L l L l l′ l*		
Amount of illusion = 4·75 mm.		Amount of illusion = 6·5 mm.	

[2] See pp. 43 ff.

TABLE I.

		Standard 30 mm.		Standard 50 mm.	
		P	*M*	*P*	*M*
			Adults		
Subject		mm.	mm.	mm.	mm.
O. T. ..	*L*	3·5	5	4·5	6·25
L. S. ...		2·75	4·25	3·5	4·5
R. K. ...		6·25	6·5	5	5·5
F. M. L.	*R*	2·25	3	3·25	4·5
B. R. ...		4	5	5·5	7
S. D. ...		5	5·25	4·5	6
	Average	4·0	4·8	4·4	5·6
	m.v. ...	1·1	0·81	0·65	0·95
			Boys		
P. B. ...	*L*	6·25	7·5	6·5	7
M. W. ...		3·75	4·25	5	6·75
S. A. ...	*R*	5·5	7·5	6	7
	Average	5·2	6·4	5·8	6·9
	m.v. ...	0·93	1·45	0·53	1·2

The most significant feature of the results given in Table I is that in each case the illusion is greater with the momentary than with the prolonged exposures. Similar results have been obtained with other illusions[1]. This fact is in direct contradiction of Wundt's explanation of the illusion. He endeavours to account for the presence of the illusion when no movements of the eye are possible, by the following considerations. In the first place, he maintains that perspective effects become more marked when the eyes are fixated on one spot, and that these effects increase the illusion. It is very improbable, however, that this secondary factor (the existence of which is very questionable) would have such a marked influence upon the amount of the illusion. Moreover, in putting forward this explanation, Wundt seems somewhat inconsistent, because, in opposition to Thiéry[2], he regards the perspective effects as dependent upon the illusion of length, which he attributes directly to the movements of the eyes. In the second place, Wundt urges that eye-movements are not necessary for the presence of the illusion. It is sufficient if the sensorial data call forth the 'disposition' to perform the movements. But, obviously, this explanation is untenable, because the mere disposition cannot be *more* effective than the actual movement, in producing the illusion.

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[1] This *Journal*, 1908, II. 294.

[2] *Op. cit.* 55.

It is interesting to note that the illusion does not increase in proportion to the length of the standard. In fact, the illusion was less with some observers when the standard was 50 mm. than when it was 30 mm. This feature seems contradictory to Lipps's view[1] that the larger the distance the greater the expansive tendency becomes. Nevertheless, introspection on the part of several of the subjects supports the view that these expansive tendencies actually exist. The subjects found that when the filled or unfilled space was directly fixed for a prolonged period, the distance seemed to expand in quite an astonishing manner, and that this influenced their judgments considerably. But it should be noted that this expansive tendency is a feature of the process of perceiving, and not of the process of imagining the figures or distances, as Lipps maintains. There is a distinct tendency to overestimate the magnitude which gives a foveal impression, and to underestimate the magnitude when it forms a peripheral impression, which may be explained by the difference of local signature in the foveal and peripheral parts of the retina.

(*b*) *The apparent exception of bisected space.* Nine subjects made observations with Fig. 3, in which the distances are marked by dots, and with Fig. 4, in which the distances are marked by lines 2 mm. long. Fig. 3 was obtained with the plate (Fig. 2) already described.

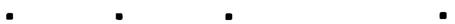

Fig. 3.

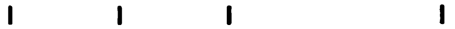

Fig. 4.

Another plate was required to present Fig. 4. The method of procedure was similar to that of the preceding series of experiments. The observations with these figures were made at separate sittings, some of the subjects giving readings with Fig. 3 first, and others with Fig. 4 first. The results obtained are given in Table II, the average illusion and the mean variation (in parenthesis) of the nine subjects being given. A positive value indicates that the filled or bisected space was overestimated, whereas a negative value indicates that it was underestimated.

The data of Table II indicate that with momentary observation the bisected spaces of both figures were overestimated, which is in accordance with the general illusion of overestimation of the filled space. The results with prolonged observation were not so uniform. Whereas, with prolonged observation the bisected space of Fig. 4 was generally overestimated, that of Fig. 3 was generally underestimated. Thus, with Fig. 3 when the standard was 50 mm. seven subjects overestimated and two underestimated the bisected space; and with the standard 30 mm. six subjects overestimated and three underestimated the bisected space. With the prolonged observation of Fig. 4, when the standard was 50 mm. all the subjects overestimated the filled space, whereas when the standard was 30 mm. seven of the subjects overestimated it, and two underestimated it.

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[1] *Op. cit.* 141.

TABLE II.

Average readings of nine subjects.

	Standard 30 mm.		Standard 50 mm.	
	P	*M*	*P*	*M*
	mm.	mm.	mm.	mm.
Fig. 3 ...	−2·0 (1·34)	2·9 (0·44)	−1·2 (1·53)	1·9 (0·57)
Fig. 4 ...	2·2 (1·58)	3·7 (0·53)	1·8 (1·06)	2·75 (0·34)

From these results it is evident that the illusion of underestimation of bisected space is not as uniform as it has hitherto been supposed to be. But in spite of this lack of uniformity in our results, we seem justified in concluding that when the two halves are clearly separated as in Fig. 4, there is a general tendency to overestimate the bisected space; whereas, when the two halves are less distinctly marked, by dots as in Fig. 3, there is a general tendency to underestimate the bisected space.

Hence, one condition of the overestimation of the filled space is the perceptual discreteness of the parts. This receives support from the introspective remarks of the subjects. One of the subjects stated that whereas he apprehended the filled space of Fig. 3 at one glance, there was 'a double act of perception' required in observing the filled space of Fig. 4. The experimenter also observed that the first few readings given at a sitting often indicated a tendency to overestimate the bisected space, but as the subject acquired a more systematic method of apprehending the figures, the tendency to underestimate it became evident. These various features strongly support the view that overestimation of the filled space is due to lack of perceptual unity, and that underestimation of the filled space results when this unity is secured[1].

This conclusion enables us to offer an explanation why the illusion of overestimation of the filled space is greater with momentary than prolonged exposures. Perceptual synthesis, more so than perceptual analysis, requires time for the adjustment of attention, and of the physiological mechanism involved. The momentary exposure allows no time for the adjustment required to effect this perceptual synthesis, with the result that the subject is influenced in his perception by the manifoldness of the parts of the filled space, which seems to cause directly the illusion of greater extensity. The simultaneity of a number of impressions does not of necessity imply that there will be perceptual unity. In order to perceive a number of objects as a unitary whole, time for systematic observation is required; and this is afforded in the case of the prolonged exposures, with the result that the illusion due to the discreteness of the parts becomes considerably diminished. The experimenter invariably noticed that the longer the subject observed the figure, the less became the illusion of overestimation of the filled space.

(c) *Effects of Practice.* The Zöllner, Poggendorff, and Müller-Lyer illusions[2] have previously been shown to disappear with practice, whereas the 'horizontal-vertical' illusion does not seem to diminish with practice[3]. The method of procedure in the investigation of this problem was similar to that adopted with the Müller-Lyer illusion[4]. A subject gave readings for several days with prolonged and momentary exposures of the figures. Only with one figure (standard 30 mm.) were complete readings taken, but the figure with standard 50 mm. was presented now and again during the sitting, in order to prevent the subject relying upon an absolute impression of the standard. One adult and one boy gave readings in this series of experiments.

The results are given in Table III. In their chief features they resemble those previously obtained with the Müller-Lyer illusion. The illusion disappears by practice with the prolonged exposures, whereas only a slight decrease is effected by practice with the momentary exposures. If we accept the suggestion that illusions which disappear with practice are due to 'psychological' factors, whilst those which do not are due to 'physiological' factors[1], this result is additional evidence against the physiological explanation of the illusion proposed by Wundt and Hering.

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[1] Rieber found when investigating the tactual form of this illusion, that when the stimulated points were very close together, there was a tendency to underestimate the filled space; but on getting the stimulating points at different temperatures, the ordinary illusion of overestimation appeared again. *Harvard Psychol. Stud.*, 1903, I. 47–99.

[2] *Psychol. Rev. Monogr. Suppl.* 1905, VII.

[3] This *Journal*, 1908, II. 243.

[4] *Ibid.* 294.

Although introspection gives no direct evidence as to the causes of the disappearance of the illusion by practice with the prolonged exposures, yet it was observed by the adult subject that after several sittings, the figure appeared to acquire a 'rigidity' which it lacked at first, and that the intermediate points seemed less obtrusive, with the result that the figure lost its manifold appearance. This 'rigidity' may be regarded as a sign that the observer had effected perceptual synthesis. As has already been pointed out, the momentary exposures do not allow sufficient time for any systematic perceptual synthesis, with the result that practice brings about no great change in the amount of the illusion.

TABLE III.

Fig. • • • • • •

Standard 30 mm. * Denotes a day's rest.

	Subject J. R. L. (adult).		Subject G. N. (boy).	
	P	*M*	*P*	*M*
Day	mm.	mm.	mm.	mm.
1......	4·25	6	6	8
2......	3	6	5·5	7·5
3......	2·5	5·5	5	7·5
4......	2·5	5·25	5·25	7·25
5......	2	4·5	*5·75	7
6......	*1·25	4·5	5	6·75
7......	0·75	4·5	4·25	6
8......	0·25	4·5	4	6·5
9......	0	4·5	4	6
10......	0·5	4·25	4·25	6·25
11......	0·25	4·5	*4·25	6
12......	*0	4·5	3·75	6
13......	0·5	4·5	3·25	6
14......	—	—	3·25	5·75
15......	—	—	3	6
16......	—	—	2·75	6

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[1] When we speak of a 'physiological' illusion, all that is meant is that the illusion can be explained by physiological factors, *e.g.* by a difference of retinal meridians. Similarly, a 'psychological' illusion simply means one that is explained by psychical factors, *e.g.* by lack of perceptual analysis. Of course, there is a physiological and a psychological aspect to all illusions.

At this point, a few of the characteristic features of the results obtained with the adults and boys may be noted. It is obvious that no general conclusions should be based upon the results of such a small number of cases as were obtained in the investigation of this illusion; but the features which we shall mention are substantiated by the results of a much larger number of adults and boys who made observations in a previous investigation of the Müller-Lyer illusion. In the first place, it may be observed that these illusions are greater with children than with adults (Table I). Secondly, the young observer as a rule requires much more practice than the adult in order to overcome the illusion. Thus, whereas the adult J. R. L. was able to overcome the illusion completely after nine days' practice, the boy G. N. showed an illusion of several millimetres even after sixteen days' practice (Table III). Thirdly, it appears that whereas the adult made greater progress after a day's pause, the boy seemed to have lost ground after missing a day's practice. This suggests that the most beneficial pauses in the acquirement of a habit are shorter for children than adults.

(*d*) *Variation of the illusion with the number of divisions.* In this part of the investigation, a large number of readings were required from the same subject, in order to find the relative amounts of the illusion with figures in which the filled spaces were divided up into 2, 4, 6, 8, and 10 equal parts. This introduces a difficulty, because the amount of the illusion is modified considerably by practice with prolonged exposures. The difficulty was overcome in two ways. Some of the subjects made all their observations with momentary exposures, in which case, practice has but little effect upon the amount of the illusion. Other subjects made observations with the prolonged exposures, allowing several days to elapse between the successive sittings, because it had been observed that very little of the effects of the practice of one sitting persisted after four or five days had elapsed.

Four subjects (adults) made observations with the five different figures mentioned above. Each subject gave complete readings with one standard only; but, as in the case of the practice series, the other standard was employed now and again. The following was the method of procedure with each of the four subjects:

Subject M. R. Observations with momentary exposures. Standard 50 mm., lying to the left of the variable. Figures observed in the order 2, 4, 6, 8, 10[1].

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[1] These numbers refer to the number of divisions in the filled space.

Subject P. R. P. Observations with momentary exposures. Standard 30 mm., lying to the left of the variable. Figures observed in the order 10, 8, 6, 4, 2.

Subject J. L. Observations with prolonged exposures. Standard 50 mm., lying to the right of the variable. Figures observed in the order 4, 8, 2, 10, 6.

Subject N. T. S. Observations with prolonged exposures. Standard 30 mm., lying to the right of the variable. Figures observed in the order 6, 10, 2, 8, 4.

TABLE IV.

Number of divisions...	2	4	6	8	10
Subject	mm.	mm.	mm.	mm.	mm.
M. B.	3·5	7	8·5	9·25	8·75
P. R. P.	2·5	4·25	5·5	6·5	5·25
J. L.	–1	3·5	5	6·25	6
N. T. S.	–1·25	2·75	4·5	4	3·25

The minus sign indicates that the filled space was underestimated.

The results given in Table IV indicate that, up to a certain point, the amount of the illusion increased as the number of divisions increased; but when there were about eight divisions, the illusion reached its maximal value in these figures, further subdivision being accompanied by a decrease in the amount of the illusion. Our results also suggest that the maximal illusion was more quickly reached when the standard was 30 mm. than when it was 50 mm.

The feature of maximal illusion is common to several other geometrical-optical illusions, and it indicates that in these cases there are at least two contrary factors operative in the perception of these figures. It has already been pointed out (p. 42) that the factor which conditions the illusion of filled and unfilled space is the perceptual discreteness of the filled space; and it is but natural that this discreteness should increase with the number of divisions. But in order that each part may be apprehended independently, its magnitude must be such as to form an appreciable part of the whole divided space. The smaller the size of the divisions, the less likely are they to receive independent attention, as there is a tendency for the small divisions to fuse together when perceived. This tendency was clearly observed by the subject P. R. P. For example, in giving readings with the figure in which the filled space was divided into ten equal divisions, he found a natural tendency to combine the parts into three groups.

Thus although the figure had ten divisions, it was perceived as one having but three divisions; and this explains the tendency of the illusion to decrease when the divisions become very small in magnitude.

(*e*) *Symmetrical and Unsymmetrical Figures.* Some previous investigators[1] of this illusion have drawn attention to the aesthetic factors which enter into the perception of the figure. This series of experiments was designed to find the part played by the aesthetic factor of symmetry. Four subjects made observations with a symmetrical figure in which the filled space was divided into five equal parts (Fig. 5), and also with an unsymmetrical figure similar to Fig. 6, in which the standard was divided into five unequal parts. The observations with the symmetrical and unsymmetrical figures were made at different sittings. The results are given in Table V.

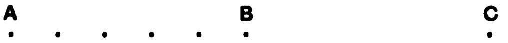

FIG. 5.

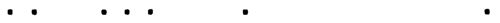

FIG. 6.

TABLE V.

Average readings of four subjects.

	Standard 30 mm.		Standard 50 mm.	
	P	*M*	*P*	*M*
	mm.	mm.	mm.	mm.
Fig. 5 ...	3·8 (0·77)	5·3 (0·82)	4·5 (0·84)	6·2 (1·5)
Fig. 6 ...	2·4 (1·12)	5·4 (0·85)	2·9 (1·43)	5·9 (0·91)

The most obvious feature of these results is that the amount of the illusion with momentary exposures is practically the same for both figures, whilst, with prolonged exposures, the illusion is considerably decreased when the figure becomes unsymmetrical. This was the case with each of the four observers. The introspective notes of the subjects suggest the explanation of this feature. With the prolonged exposures, the subjects experienced greater difficulty in forming judgments with the unsymmetrical than with the symmetrical figure.

Now the attitude of comparative ease in forming a judgment, which the symmetry of the figure naturally favours, is that most conducive to the experiencing of an illusion in a marked degree; whereas the attitude assumed when the observer is confronted with the difficulty of perceiving an unsymmetrical figure, on the other hand, results in greater cautiousness, and leads him to avoid or to overcome the factors which produce the illusory effects, although he may not be aware of the precise nature of these factors. The considerable time required to form a satisfactory judgment on the unsymmetrical figures, and the greater effort called forth, enable the observer to overcome more or less successfully the obtrusiveness of the points of the filled space. In the case of momentary observations there is no time for the secondary factor of symmetry or lack of symmetry to influence the act of perception, with the result that there is no marked difference in the amount of the illusion with the symmetrical and unsymmetrical figures.

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[1] Rieber, *Harvard Psychol. Stud.*, I. 47–99.

IV. SUMMARY.

The results of this investigation throw some light upon the chief differences between prolonged and momentary observation. The factors which come into play in momentary observation appear to be simpler and more constant than those in prolonged observation.

For example, practice with prolonged observations produces a marked decrease in the amount of the illusion, whereas practice with momentary observations produces very little change. Again, the amount of the illusion is considerably modified in prolonged observation if the symmetry of the figure is disturbed, but this produces very little change in momentary observation. Another instance of this difference is that the exception of the underestimation of bisected space occurs only in prolonged exposures, whereas filled space is invariably overestimated in the case of momentary exposures. These facts seem to point to the conclusion that the method of momentary observation resembles the simplest and most naïve form of perception; and it is by the careful study of this method of perception that we shall most probably arrive at the fundamental features of visual spatial perception. In prolonged observation, on the other hand, various secondary factors come into play, and these to all appearances tend to obscure the innate discriminative sensibility, which is compensated by more systematic methods of apprehension and by the richer significance which the figures acquire.

We will summarise briefly how these results appear to substantiate or to contradict the various explanations of this illusion which have hitherto been proposed. It is obvious that the definite increase of the illusion, as the period of observation becomes shorter, is directly opposed to Wundt's general standpoint in explaining geometrical-optical illusions. Several of our results are in accordance with Hering's general theory of spatial perception; but his explanation of this particular illusion is not substantiated by these results. The illusion seems much greater than could possibly be explained by the difference between the curvature and the chords of the retina. Moreover, Hering's theory does not seem to give a satisfactory explanation of the feature of maximal illusion. Lipps's view that the illusion is caused by the 'expansive tendencies' of the image which is formed of one part of the figure and which is superposed upon the part of the figure actually perceived at a certain moment, is refuted by the fact that the illusion is very pronounced with momentary exposure when no time is allowed to superpose the image of one part of the figure upon the other part. Introspection on the part of the subjects, however, showed that 'expansive tendencies' have considerable influence upon the perception of these figures. The distance between two points appeared distinctly greater when directly than when indirectly apprehended. The explanation of this may possibly be found in differences of local signature in the foveal and peripheral parts of the retina.

Passing to the positive significance of our results, we must emphasize the fact that the illusion is most marked in momentary observation, which suggests that the explanation of the illusion is to be sought among the fundamental factors of spatial perception and not among the secondary factors. We have already noted that to attribute the illusion to the physiological factors or the sensorial data as Wundt and Hering have done, seems unsatisfactory. The disappearance of the illusion with practice is, *prima facie*, an argument against any physiological explanation. The general results of this investigation lead us to seek an explanation rather in the nature of that synthesis or lack of synthesis which characterizes our crudest perception. One fact that has become evident is that the discreteness of the various parts of the filled space is an essential factor in this illusion. For instance, it was shown that bisected space, although generally underestimated, comes to be overestimated, as all other filled spaces, when the two parts are clearly separated by a sufficiently pronounced mark. The discreteness of the parts gives rise to a 'feeling of manifoldness' which directly causes an exaggerated judgment of extensity. The independent apprehension of the various parts, whilst the subject is forming a judgment of the length of the whole filled space, inevitably involves the tendency to contrast the length of the whole with that of the part; and this, as Helmholtz pointed out, is one factor of the illusion. Moreover, we have here some evidence in support of a suggestion made in our discussion in a recent article on the Müller-Lyer illusion[1], namely, that perceptual dissociation and contrast effects are causally related.

The presence of the feature of maximal illusion also points to the importance of the independent apprehension of the parts of the divided whole. The illusion increases with the number of parts, until the parts become too small to claim independent attention. Thus, when the standard was divided into ten parts, the small divisions tended to fuse together into two or three groups, a tendency which was naturally accompanied by a decrease in the amount of the illusion. This grouping of the parts was also very easily effected in the case of unsymmetrical figures (Fig. 6), which partly accounts for the small illusion with such figures. Another important factor of perceptual synthesis is the systematic relating of the parts of the figure to some one point. For instance, the underestimation of the bisected space is to be attributed to the fact that the attention of the subject is held by the middle point, and this acts as a 'centre of reference' to which the two parts become related. Hence it appears that simultaneity of impressions is not the most important condition of perceptual synthesis. System, not simultaneity of impressions, characterizes the higher forms of perception.

This further study suggests that psychological geometrical-optical illusions may be classified according as they are caused by lack of perceptual analysis or by lack of perceptual synthesis. Thus the Müller-Lyer illusion and the illusion of 'filled and unfilled space' may be regarded as converse illusions. In the Müller-Lyer illusion the magnitude of the part is influenced by the magnitude of the whole, the figure being so constructed as to prevent the independent apprehension of the part. In the illusion of 'filled and unfilled space,' on the other hand, the perception of the whole is modified by the obtrusiveness of the parts, which gives rise to contrast effects, and also prevents the observation of the filled space as a unitary whole.

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[1] This *Journal*, 1909, III. 40.

(*Manuscript received December* 13*th*, 1911.)

GENERAL ABILITY, ITS EXISTENCE AND NATURE.

By BERNARD HART AND C. SPEARMAN.

1. *The fundamental question in psychological correlation.*
2. *The proposed criterion.*
3. *The experimental results required by the three theories respectively.*
4. *Confrontation of the three theories with all the facts.*
5. *Evidence of 'Mental Tests.'*
6. *Some inadequate explanations of the General Factor.*
7. *Explanation by 'Intellective Energy.'*
8. *Physiological considerations.*
9. *Importance of the specific abilities.*
10. *Applications of the doctrine of general and specific abilities.*
11. *Conclusions.*

1. *The fundamental question in psychological correlation.* "Si jamais question fut controversée, c'est bien celle de la valeur des corrélations. Deux opinions absolument contradictoires sont en présence, et toutes deux revendiquent la force des preuves." Such was the recent conclusion of Binet as to the interpretation of psychological correlations[1].

As representative of the one opposing side, he cites Thorndike, for whom "l'esprit ne serait qu'une collection absolument hétéroclite de facultés qui sont comme juxtaposées, mais restent rigoureusement indépendantes." According to this view, ability in any performance depends upon a complex of elementary factors; the correlation between two performances simply measures the degree in which the elementary factors demanded by the one happen to coincide with, or to be bound to, those demanded by the other. The elementary factors include both

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[1] *Les idées modernes sur les enfants*, 1909, 242.

'form' and 'content'; by form is meant the kind of mental operation, as discrimination, observation, inference, etc.; while the 'content' denotes the different sorts of data, as colour, shape, number, etc., submitted to such operations[1].

Among the upholders of the other side, Binet quotes one of us. Here, the view supported is that all performances depend to a certain degree upon one and the same *general* common factor, provisionally termed 'General Ability.' Correlations are thus produced between all sorts of performances, the amount of correlation being simply proportional to the extent that the performances concerned involve the use of this general common factor, or 'General Ability.'

To these two extreme views cited by Binet may be added a great number of intermediate ones, which, however, seem to differ from one another more in detail than in principle. Take first those writers who still treat discrimination, imagination, etc. as isolated 'faculties': they obviously imply, however tacitly, that individuals excelling in one kind of discrimination must also excel in other kinds; they assume, in fact, correlation between all performances belonging exclusively to the same 'faculty,' and absence of correlation between performances belonging wholly to different 'faculties.' And the same applies, even though modified quantitatively, to the very numerous supporters of mental 'types'; when they classify people as 'visual,' 'auditory,' 'motor,' etc., or in any other more ingenious manner, they evidently imply that the different kinds of visual abilities tend to go together (except in so far as their 'types' merely refer to habits and preferences, not abilities). It is interesting to notice that even Thorndike appears to be surrendering his former belief in the complete independence of all mental powers, and to be coming round to an opinion of the same class as those just mentioned. In place of 'faculties' or 'types' he introduces the more cautious term of 'levels'; these he declares to be three in number, namely, sensitivity, association, and dissociation; he says that performances correlate highly with those belonging to the same 'level,' lowly with those belonging to a different one[2].

In all the views quoted in the last paragraph, each different 'faculty,' 'type,' or 'level' is regarded as producing a separate focus of correlation; all may be classed together as 'multifocal' theories. The theory of a general common factor, on the other hand, may be called

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[1] *Amer. J. of Psychol.* 1909, xx. 368.

[2] *Educ. Psychol.* 1910, 191.

'unifocal.' And lastly, Thorndike's old view of universal independence is clearly 'non-focal.'

This sharp divergence between the three current views appears to be of grave importance. It bars the way to all interpretation of our laboriously accumulated correlational data. It confuses all theory as to the intellectual 'make-up' of individuals. And it paralyses our practical power of gauging the intelligence of persons, both normal and insane.

Hence, we believe that we are doing a useful service in bringing forward a criterion which is crucial, and yet so simple as to require no mathematics beyond the ordinary calculation of correlational coefficients. The criterion enables any one to tell at once which of the three views is in accordance with the actual observations.

2. *The proposed criterion.* As is now well known, the correlation between any two performances (or other variables) may be estimated quantitatively by means of a 'coefficient.' Several such coefficients have been devised, but the one most used is the Bravais-Pearson, denoted by the letter r. When excellence in one performance is strictly proportional to excellence in another, the correlation between them is said to be complete, and $r = 1$. When, as usual, there is merely a *tendency* for excellence in the two to go together, then r becomes a fraction, which is smaller in proportion as the tendency is slighter. When the two are quite independent, $r = 0$. When ability in one performance goes with inability in another, the correlation is called inverse, and r becomes minus, up to the limit of -1[1]. When such coefficients have been calculated between a number of performances, all the results may be drawn up in a table. An example is given in Table I (the decimal points being omitted)[2].

Now, the three conflicting theories evidently involve different general relations between the coefficients in such tables. The desired crucial criterion will be attained, if we can find any function of these coefficients that will take markedly different values according to the different theories. For then we have merely to determine the value of this function from any actual observations and to see with which theory it agrees, if any.

The principal difficulty lies in the 'errors of sampling.' If a few dozen tosses are made with dice, the number of times that any face comes uppermost evidently cannot be taken as a perfectly accurate measure of its real probability of doing so; the tosses will only have supplied a more or less approximate 'sample' of this probability; it will have been liable to considerable falsification by the innumerable minute uncontrollable influences which we call 'chance.' Such sampling errors will disturb our correlational coefficients and any function of them. The general magnitude of this disturbance by chance diminishes as the 'sample' becomes larger, and it admits of precise estimation by means of the 'probable error.' In no theory, then, can conformity to observation be expected to surpass the limits prescribed by the probable errors of sampling.

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[1] For method of calculating r, see Yule's *Introduction to the Theory of Statistics*, 1911, ch. XI. A brief description is given in Myers's *Text-book of Experimental Psychology*, ch. X.

[2] From Burt, this *Journal*, 1909, III. 161.

TABLE I.

Performances	Dotting	Alphabet	Sorting	Imputed Intell.	Dealing	Spot pattern	Tapping	Mirror	Sound	Lines	Touch	Memory	Weight
Dotting		77	67	60	69	57	57	50	52	48	38	20	16
Alphabet..... ...	77		74	61	66	59	53	29	52	16	62	31	07
Sorting	67	74		52	72	45	61	34	52	14	22	19	23
Imputed Intell.	60	61	52		44	76	47	67	40	29	13	57	-13
Dealing	69	66	72	44		51	65	40	34	47	23	19	01
Spot pattern ...	57	59	45	76	51		41	45	47	25	03	26	11
Tapping	57	53	61	47	65	41		45	47	08	26	-05	22
Mirror............	50	29	34	67	40	45	45		34	16	08	05	-05
Sound	52	52	52	40	34	47	47	34		-07	-01	01	-13
Lines	48	16	14	29	47	25	08	16	-07		26	06	19
Touch............	38	62	22	13	23	03	26	08	-01	26		16	29
Memory	20	31	19	57	19	26	-05	05	01	06	16		05
Weight	16	07	23	-13	01	11	22	-05	-13	19	29	05	
	611	587	535	533	531	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]

To minimise this disturbance by mere chance, it is necessary to make the criterion as comprehensive as possible. No reliable conclusion can be expected from picking out—as some authors have done—one or two particular coefficients from the table to serve as sole basis of argument. The most obvious method would be to devise as criterion some direct function of all the coefficients in the table. We have, however, chosen a somewhat different course. It seemed desirable to retain the power of noting whether the whole table obeyed the same law or different parts of it behaved differently. Also, we were anxious to simplify the calculations as far as possible, in order to appeal to a wider circle of readers.

For these reasons, our criterion was based upon singling out from the table any *pair of columns of coefficients* (of course *rows* would come to the same thing). For instance, let us take from Table I on page 54 the pair of columns for Touch and Memory; one coefficient in each column has no correspondent in the other and therefore has to be omitted. We get the following figures:

Touch	Memory
·38	·20
·62	·31
·22	·19
·13	·57
·23	·19
·03	·26
·26	−·05
·08	·05
−·01	·01
·26	·06
·29	·05

Our criterion consists simply in the correlation between one column of figures and the other; it is the correlational coefficient between the two series of correlational coefficients; clearly, this is just as easy to work out as between any other two series of values. It should be noted that this correlation between columns is quite independent of the arrangement in which the table happens to have been drawn up.

To secure still greater comprehensiveness, in every table examined by us we have 'pooled[1]' the correlations between the different pairs of columns and have based all consideration upon the mean value. Thinking that the results might possibly be different according as the sum total of the pair of columns was of large or small magnitude[2], we have pooled into one mean the results for the five pairs of columns whose sum totals were highest, and into another mean those for the five pairs whose sum totals were lowest. But in so doing, we took only those columns which came up to our correctional standard of reliability, as indicated in the following paragraph.

One further precaution is absolutely indispensable. The above mentioned sampling errors may possibly introduce into our criterion, not only random irregularities, but also a definite bias. Until this bias has been allowed for, consideration of the coefficients may be grossly misleading. This necessitates more elaborate calculations; our process of estimating and correcting the bias is given in Appendix II. Fortunately, in some instances the probable errors are so small, that the process of correction can be dispensed with. In any case, the correction must be kept within limits; as usual, the larger the correction, the less it is to be trusted. If the sampling errors are big enough, they eventually will quite swamp the true differences of magnitude upon which the desired correlation should be based. In this case, the true correlation is beyond ascertainment; any attempt at correction is merely illusory. To avoid this, and at the same time to secure impartial treatment of all data, it is necessary to fix beforehand some definite limit to the feasibility of correction. We have here adopted the following standard: in order to attempt to estimate the correct correlation between columns, *it is required that in each of these columns the mean square deviation should be at least double the correction to be applied to that deviation.* Roughly speaking, this is equivalent to requiring that the mean square probable error of the coefficients in any column should not be greater than a quarter of the mean square deviation of those coefficients. Evidently, the above standard may be unattained for two very different reasons: the largeness of the probable errors, or the smallness of the mean deviation[1]. The very small deviations are generally found in the columns which have the smallest sum totals, so that these columns are rarely usable; their true values are swamped by their sampling errors.

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[1] That is, taken the average of.

[2] We get this sum total by adding together all the values in both columns, regardless of sign. For instance, in the above pair of columns it amounts to 4·45.

It should be noted that the biggest possible value of a correlation by definition equals 1. It might therefore seem a contradiction if our corrected value ever exceeded 1. But it must be remembered that every calculated value, whether corrected or not, equals the true value *plus* an error (from sampling or otherwise). The best mode of calculation is plainly that which, when applied to different samples, gives values deviating as little as possible on an average from the true value. To secure this, the deviation must necessarily tend equally to be positive or negative (a well-known theorem in probability[2]). Hence, should the true value happen to be very near to $+1$ or -1, the calculated value would almost as often exceed 1 as not.

3. *The experimental results required by the three theories respectively.* Thorndike's former 'non-focal' theory asserts that, in general,

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[1] So that the method is inapplicable to the special case that $r_{ap}/r_{aq} = r_{bp}/r_{bq} = 1$. Here another method is needed, as given by Abelson in the last number of this *Journal.*

[2] See Fechner, "Ueber den Ausgangswerth der kleinsten Abweichungensumme, etc." (*Abh. d. kgl. sächsischen Gesellschaft d. Wissenschaften*, 1878, XVIII.).

the coefficients in the table are all independent of one another. Hence, it is at once plain that, if this theory is true, the correlation between columns should approximate to 0.

Quite different are the results needed to accord with the 'multi-focal' theory implied in 'types,' 'faculties,' and 'levels.' For illustration, let us assume, with Thorndike, that there are three levels, sensitivity, association, and dissociation, which we will denote by *S*, *A*, and *D*. We will suppose that three tests have been selected from each level, and will indicate these by the indices 1, 2 and 3. The theories of this class claim that the only interdependence between the coefficients consists in the correlations being high between two tests on the same level, but low between those on different levels. If we denote high coefficients by *h* and low ones by *l*, the table of coefficients may be written as follows:

	S_1	S_2	S_3	A_1	A_2	A_3	D_1	D_2	D_3
S_1		*h*	*h*	*l*	*l*	*l*	*l*	*l*	*l*
S_2	*h*		*h*	*l*	*l*	*l*	*l*	*l*	*l*
S_3	*h*	*h*		*l*	*l*	*l*	*l*	*l*	*l*
A_1	*l*	*l*	*l*		*h*	*h*	*l*	*l*	*l*
A_2	*l*	*l*	*l*	*h*		*h*	*l*	*l*	*l*
A_3	*l*	*l*	*l*	*h*	*h*		*l*	*l*	*l*
D_1	*l*	*l*	*l*	*l*	*l*	*l*		*h*	*h*
D_2	*l*	*l*	*l*	*l*	*l*	*l*	*h*		*h*
D_3	*l*	*l*	*l*	*l*	*l*	*l*	*h*	*h*	

The correlation between columns will evidently be of two kinds as shown in the following two examples:

Pairs of columns belonging to the same level

S_1	S_2
h	*h*
l	*l*
l	*l*
l	*l*
l	*l*
l	*l*
l	*l*

Pairs of columns belonging to different levels

S_1	A_1
h	*l*
h	*l*
l	*h*
l	*h*
l	*l*
l	*l*
l	*l*

When the columns belong to the same level, the high coefficients will come in corresponding places, so that the correlation between the columns will be positive. When the columns belong to different levels, the high coefficients always come in non-corresponding places, so that the correlation between the columns is negative. As there are more pairs of columns belonging to different levels than to the same one,

one would expect the mean value of the correlation to be low and negative. And this may be demonstrated strictly (see App. V.).

Just as the above two theories differ radically from one another in their effect on our criterion, so again the theory of a General Common Factor departs widely from either of them. The effect of this theory on the coefficients has been expressed in various ways, all, however, really equivalent to one another[1]. The way most often cited is the possibility of arranging the coefficients in a 'hierarchy'; by this is meant, that the values of the coefficients are highest in one corner and thence gradually diminish in both vertical and horizontal directions, the diminution of every column (or row) being in the same proportion[2]. But this method has proved ineffective; authors have been liable to arrange their tables arbitrarily; and their estimates as to how far the hierarchy held good have tended to be mere assertions. An expression of exactly the same system of relations, but one admitting of precise quantitative corroboration, is given by the equation

$$\frac{r_{ap}}{r_{aq}} = \frac{r_{bp}}{r_{bq}} \quad \dots\dots\dots\dots\dots\dots\dots\dots\dots\dots (1),$$

where a, b, p, and q represent any four of the performances in question; r_{ap} is the Bravais-Pearson coefficient between a and p; similarly, as regards r_{aq}, r_{bp}, r_{bq}[3]. Now, this equation causes the correlation between every pair of columns to amount to exactly + 1; this can be seen even on inspection, but a formal proof is given in Appendix I.

Here, however, consideration must be turned to a point which appears to have given rise to misunderstanding. The opponents of the theory of a General Factor have taken this as claiming to be the *sole*

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[1] *Amer. J. of Psychol.* 1904, xv. 272; *Ztsch. f. Psychol.* xliv. 85; this *Journal*, 1909, iii. 159.

[2] *Amer. J. of Psychol.* 1904, xv. 274.

[3] See Burt, this *Journal*, 1909, iii. 159. The demonstration previously offered for this equation, based on the formula for eliminating chance errors of observation (Krueger and Spearman, *Ztsch. f. Psychol.* 1907, xliv. 84), has apparently not been found easy to understand. We therefore give a new very simple and stringent proof, based this time on Yule's well-known formula for 'partial' coefficients.

Let r_{ag} and r_{pg} denote the correlations of a and p with the theoretical General Factor. And let $r_{ap.g}$ denote the correlation that a would have with p if the influence of the General Factor were eliminated. Then, by Yule's formula

$$r_{ap.g} = \frac{r_{ap} - r_{ag}r_{pg}}{\sqrt{(1 - r_{ag}^2)(1 - r_{pg}^2)}}.$$

But by definition of r_{ap}, $r_{ap.g} = 0$, so that $r_{ap} = r_{ag}r_{pg}$, and similarly $r_{bp} = r_{bg}r_{pg}$. Hence, $r_{ag}/r_{bg} = r_{ap}/r_{bp}$ and similarly $= r_{aq}/r_{bq}$, which gives us at once the above equation (1).

source of correlation. Such an absurd claim does not seem really to have been advanced by any one. The earliest announcement of the principle was accompanied by a warning of "its inevitable eventual corrections and limitations[1]." Special emphasis was laid on the fact that correlation between performances is also produced by great similarity between them. Obviously, as the similarity tends towards completeness, the correlation must tend towards unity. This fact was underlined by actual examples in numerical detail. For instance, the correlation between Latin translation and Latin grammar was shown to be far too large to fit into the theory, and this was attributed to the content being the same in both cases, namely, Latin[2]. Another instance of the same sort was French prose and French dictation[3]. A further one was furnished by the test of counting letters one at a time and that of counting them three at a time; here, there was a close similarity both of content and form, and accordingly this was pointed out as the cause of the principle becoming invalid[4].

It was never asserted, then, that the General Factor prevails exclusively in the case of performances too alike: it was only said that *when this likeness is diminished (or when the resembling performances are pooled together), a point is soon reached where the correlations are still of considerable magnitude, but now indicate no common factor except the General one.* The latter, it was urged, produces the basal correlation, while the similarities merely superpose something more or less adventitious. Up to the present, however, these similarities seem to have exercised surprisingly small influence. In all the performances dealt with in the next section, only three times did any of them resemble each other closely enough to require pooling; these cases will be discussed in detail later on.

4. *Confrontation of the three theories with all the facts.* Such, then, is the statistical method which we have devised for deciding between the three rival theories. If the older view of Thorndike, viz. a general independence of all correlations, holds good, our correlation between columns of correlational coefficients should average *about* 0. If his newer view of 'levels,' or the almost universal belief in 'types,' is correct, then the mean correlation between columns should be a *low minus value*. If, finally, the true theory is that of a General Common Factor, the correlation between columns should be *positive and very high.*

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[1] *Amer. J. of Psychol.* 1904, xv. 273.
[2] *Ibid.*
[3] *Ibid.*
[4] *Ztsch. f. Psychol.* 1907, XLIV. 102.

The values reached are given in Table II. This rests on a broad basis. It forms a summary, as exhaustive as we have been able to make it, of all the work bearing seriously on the matter. It dates from various periods, up to 30 years back. It comes impartially from strong supporters of each of the three conflicting theories, and also from authors who had not heard of the point at issue. We find that all this testimony, when at last submitted to a crucial criterion, become as concordant as could be desired. From beginning to end, the correlation between columns is positive and very high; *the mean is almost complete* + 1. This is just the value demanded by the theory of a General Factor, and as far as it possibly could be from the values demanded by either of the other two theories.

No one, it is pleasant to notice, has really furnished more valuable evidence than those who have hitherto been reluctant to admit the theory which has just proved true. It was not corroborated, for instance, by Brown, who reached the following conclusion: "The first thing to be noticed in the groups of coefficients...is that *not one of them shows the hierarchical arrangement*[1]." Our table of definite measurements, however, reveals that every single one of his groups really furnishes an almost perfect correlation between columns (and therefore correspondingly perfect hierarchy). Brown goes on to say: "it is a very significant fact that the group which approaches it most nearly...is the group where 'spurious correlation' due to heterogeneity of material was to be suspected." In reality, the particular group (our number 9) thus singled out as coming nearest to the hierarchy is the sole one that distinctly falls short of it, the mean correlation for the smallest pairs of columns being in this case only + ·73; whereas the group (our number 11) said by him to be "as homogeneous as could possibly be expected or desired," gives the closest approximation of all, the values being + ·98 and + ·96.

And as regards Thorndike himself, the figures are equally striking. It is to his laboratory and supervision that we owe the specially valuable results of Bonser. The significance of these may be demonstrated even more clearly by pooling together the results of boys and girls[2], so as to get the large number of 757 individuals and the corresponding decrease of sampling errors. In this way, we get the opportunity of testing the rival theories with no appreciable need of theoretical correction. At

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[1] This *Journal*, 1910, III. 313. The italics are his.

[2] Their respective coefficients show no discrepancies that would form any legitimate objection to this pooling.

TABLE II. *Correlation between columns in hitherto published tables of correlational coefficients*[1].

		Description of experimental series	For the 5 largest pairs of columns (or as many as are up to our correctional standard)[2]			For the 5 smallest pairs of columns (or as many as are up to our correctional standard)[2]		
			No. of pairs available	Correlation between columns: Raw	Correlation between columns: Corrected	No. of pairs available	Correlation between columns: Raw	Correlation between columns: Corrected
1.	1889.	University students, 9 men and 1 woman. Oehrn, *Ztsch. f. Psychol.* XLIV. 89	1	+·93[3]				
2.	1904.	High class preparatory school, 37 boys, ages 9—13. Spearman, *Amer. J. of Psychol.* XV. 75	1	+·89	+1·16[4]			
3.	1904.	Village school, 24 boys and girls, ages 10—13. Spearman, *Amer. J. of Psychol.* XV. 246 ff.	2	+·72	+1·01[4]			
4.	1906.	University students, 11 men. Krueger and Spearman, *Ztsch. f. Psychol.* XLIV. 75	1	+·96[3]				
5.	1908.	Normal school students, 96 persons. Peterson, *Psychol. Rev.* XV. 323	2	+·94[5]				
6.	1909.	Insane patients, 15. Foerster and Gregor, *Monat. f. Psychiat. u. Neurol.* XXVI. 42	2	+·79	+1·12[4]			
7.	1909.	Elementary school, 30 boys, ages 12½—13½. Burt, This *Journal*, III. 94	5	+·59	+·67	4	+·81	+1·14[4]
8.	1909.	High grade school, 13 boys, ages 12½—13½. Burt, This *Journal*, III. 96	5	+·65	+·63	4	+·91	+1·14[4]
9.	1910.	Elementary school, 56 boys, ages 11—12. Brown, This *Journal*, III. 296	5	+·76	+1·00	5	+·58	+·73
10.	1910.	Elementary school, 39 girls, ages 11—12. Brown, This *Journal*, III. 310	4	+·78	+1·02[4]			
11.	1910.	High grade school, 40 boys, ages 11—12. Brown, This *Journal*, III. 311	5	+·87	+·98	5	+·80	+·96
12.	1910.	University students, 23 women. Brown, This *Journal*, III. 313	5	+·89	+·97	5	+·72	+·88
13.	1910.	4th, 5th and 6th grades pub. schools, 385 boys, ages 8—16. Bonser, *Reasoning ability of children*	4	+·98[5]		2	+·90[5]	
14.	1910.	As in above. 372 girls	1	+·96				
		Mean =			+·95			+·97

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[1] Some of these tables contained the correlational coefficients, not only between the experimental performances, but also between these and an estimate of the subjects' general intellect. We have left this estimate untouched. If it is withdrawn, so as to confine the table strictly to experimental data, the only appreciable difference occurs in 7 and 8, in which the corrected correlation between columns now rises to nearly +1 (see p. 74 of this paper).

[2] For this standard, see p. 56. Where there were less than 10 pairs of columns up to standard, the pairs of medium size could be classed either among the smallest or among the largest. We have put them with the largest.

[3] Our correctional standard was not quite reached by any pair of columns in these series; but as they have previously been quoted with reference to this question, we give here the uncorrected value for the pair of columns approaching this standard most nearly. The question has also been discussed with reference to a series by Thorndike, Lay, and Dean (*Amer. J. of Psychol.* 1909, XX. 364); but this series cannot be utilised here, as it contains only two performances besides the estimates. It may, however, be said that the conclusions of these authors on the present topic seem to be groundless, being based on assuming no special correlation, either of the teachers' estimates with those of the fellow-pupils, or of the two sensory tests with one another. Both assumptions appear erroneous, see this paper, p. 74.

[4] As regards these coefficients turning out larger than the real maximum, 1, see p. 56. About half of them were bound to turn out larger if the General Factor were solely operative.

[5] In these cases the nature of the published coefficients was not fully described, nor were the probable errors given, so that we could not correct for errors of sampling. To determine which pairs of columns are up to our standard, we have—in default of better information—used the probable errors of the ordinary coefficient.

the same time, we can see whether any of the theories is applicable even to such small differences among the coefficients as occur in this investigation. The results are given in Tables III and IV. It turns out that every single usable correlation between columns, *even though uncorrected*, is almost exactly +1.

TABLE III. *Coefficients of Bonser, boys and girls pooled together*[1].

	Mathemat. Judgment	Controlled Associat'n	Literary Interpret.	Selective Judgment	Spelling
Mathematical Judgment ...		·485	·400	·397	·295
Controlled Association......	·485		·397	·397	·247
Literary Interpretation ...	·400	·397		·335	·275
Selective Judgment	·397	·397	·335		·195
Spelling	·295	·247	·275	·195	

TABLE IV. *Detailed list of all the usable*[2] *correlations between pairs of columns in Table III.*

		Correlation (uncorrected)
Mathematical Judgment *v.*	Controlled Association	+1·00
" "	Literary Interpretation	+1·00
" "	Selective Judgment...............	+ ·98
Controlled Association *v.*	Literary Interpretation	+ ·99
" "	Selective Judgment...............	+1·00
Literary Interpretation *v.*	Selective Judgment...............	+1·00

We have attempted a further refinement. As said above, the correlation between columns will be +1 whenever the equation (1) on p. 58 above is perfectly satisfied. But the correlation is somewhat more general than the equation, and may possibly be +1 even when the equation is satisfied imperfectly. As it is important to learn whether there exists the strict proportionality implied in the equation, we have worked out a further coefficient which only becomes +1 when the equation obtains exactly[3].

This 'proportionality coefficient,' as we may call it, has the additional advantage of being applicable to tables of coefficients made from only four performances, whereas the previously used ordinary correlational coefficient between columns requires at least five performances[1]. This enables us to submit two more experimental series to our criterion. One is that of Aiken, Thorndike, and Hubbell on 160 boys and girls of the 8th school grade[2]; the other is a series by Brown on 56 women students of a Training College[3]. The results are given below, the fresh series being indicated by the initials of their authors, while the remaining series are denoted by their numbers in Table II.

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[1] See his averages in tables LVI and LVII on p. 95 (*op. cit.*). 'Mathematical judgment' represents the mean result of his tests I and II; 'Controlled association' does the same for III and IV; 'Selective judgment,' for V and VI. VII is 'Literary interpretation.' As regards the taking of means, see this paper, p. 55.

[2] That is, all which come up to our correctional standard. This includes all except those with the column for Spelling.

[3] Proof is given in Appendices III and IV.

TABLE V. *The mean 'proportionality coefficients' between pairs of columns, corrected (except in the case of A., T. and H.'s series).*

Proportionality coefficient for five largest pairs of columns (or as many as are up to standard)—

Series	A. T. H.	2	3	6	7	8	9	10	11	12	B.
	+1·00[4]	+1·03	+·90	+1·18	+·66	+·67	+1·07	+·99	+·97	+·94	+·89

Proportionality coefficient for five smallest pairs of columns—

Series	A. T. H.	2	3	6	7	8	9	10	11	12	B.
	—	—	—	—	+1·07	+1·00	+·73	—	+1·06	+1·00	—

General mean = +·95.

Once more, all the values are positive and very high. Altogether, Tables II—V, the condensed expression of the work of 14 experimenters on 1463 men, women, boys, and girls, sane and insane, appear to present a unanimity of evidence not easily matched. Throughout, the most complete support is given to the theory of the General Common Factor, while the rival theories are contradicted as decisively as they possibly could be.

5. *Evidence of 'mental tests.'* It is well known that among the earliest experimental performances were those introduced under the name of 'mental tests.' A performance was thus called when its importance did not lie in itself but in the fact that it tested capacity for all other performances of the same general form. After a brief

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[1] On looking at any table of coefficients for 5 performances, it will be at once evident that the columns to be correlated consist of only 3 values each, which is the minimum for determining a correlation. No reader will be surprised at our utilising correlations between series of only 3 values, if he remembers that the correlation merely expresses the fact that the 3 values (measured from their mean) in the one column are proportional to those in the other column.

[2] *Psychol. Rev.* 1902, ix. 374.

[3] This *Journal*, 1910, iii. 312.

[4] This value has had to be derived solely from the coefficients for 'efficiency' in marking misspelled words, in marking *r* and *e*, in Association, and in Addition. The authors give some other coefficients, but too irregularly for use.

career in various countries, these tests collapsed into universal disrepute. But though subsequent psychological literature has been ready enough to condemn them, it has been curiously unable to point out their real source of weakness. This was simply the fallacy of assuming that whatever is true of any one performance is necessarily also true of all other performances usually bearing the same name, such as 'apprehension,' 'discrimination,' 'judgment,' etc.; it was the old theory—so easy to scotch, but hard to kill—of 'faculties.' This fallacy, though long banished from open daylight, still strangely persists in dark corners; it even secretly governs the thoughts of many who believe themselves to be strongly opposing it; and it appears to have been the real source of all the above discussed 'multifocal' theories of correlation, whether these openly spoke of 'faculties' or took refuge in 'types' or 'levels.'

Recently, however, tests have had a remarkable revival; their new developments (especially those proceeding from Binet) are attracting very serious attention. Indeed, after such investigations as those of Goddard, for instance[1], it seems scarcely rash to predict that tests will eventually be regarded as one of the really great achievements of modern applied psychology.

If, now, we look into the cause of this change, we find that the newer tests differ from the old ones both in purpose and in method. Their purpose is no longer, either explicitly or implicitly, to gauge any 'faculties' such as apprehension, discrimination, etc., but instead to evaluate *general ability*. In other words, the mental tests have turned from failure to success on their deserting—with however little acknowledgment—the multifocal for the unifocal theory.

Can this [illegible]

And the same holds good of their change of method. Binet's series of tests are no longer selected, as was done before, by a rigid analysis of the intellectual operations involved, but instead have about as miscellaneous a character as could well be imagined. Moreover, the evaluation is never based on the result of any single test, but on the collective result of a motley group of tests. All this procedure is exactly that required by the unifocal theory of the General Common Factor. In the collective result of many diverse performances, all the independent specific factors must tend to cancel one another, while the General Factor is every time reinforced and so in the end predominates alone.

On the whole, then, the extraordinary regeneration of mental tests seems to be due to the fact that at last both in purpose and in method they have abandoned the multifocal theories for that of a General Common Factor. And the rapidity of their future development will probably depend on the latter theory being more explicitly avowed and systematically utilised.

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[1] See especially *Ped. Sem.*, June, 1911.

6. *Some inadequate explanations of the General Factor.* So far, all is plain sailing enough. The trouble is not so much in proving the existence of the General Factor as in revealing its precise nature.

We need not dwell on the suggestion advanced, that this is really 'spurious' and merely due to the 'heterogeneity' of the experimental subjects. For heterogeneity—apart from the erroneous assumption that it necessarily involves spuriousness—has already (p. 60) been shown to have nothing to do with the General Factor.

An extraneous influence that has found more advocates is the varying ability of the subjects to understand their instructions. According to this, the same subject will tend to succeed in all sorts of tests, not through greater real power to execute them, but through more accurate comprehension of what they are required to do. But this notion has never been supported by any actual evidence, even in the case of 'mass' experiments, where alone it might conceivably exert some influence. And consideration of experimental results in detail has always led to its entire rejection[1].

Much less trivial would be the supposition of influence from the subjects' varying degrees of goodwill. Suppose that some of them tried much more seriously than others to shine in the tests, and that this was the sole cause of correlation. The effect would be exactly such correlational values as we have actually found. But as no one appears to have yet advanced this explanation, we may content ourselves here with mentioning that several investigators have brought solid evidence against it[2]. Its rôle can at most be very subordinate.

Still more serious appears a further influence not suggested by previous authors; this is the chance error of observation. One might easily enough suppose that the correlations standing lowest in the 'hierarchy' are simply those derived from the performances measured most unreliably. Decisive against this, however, is the fact that rank in the 'hierarchy' is actually found to follow quite a different order from that of rank in reliability. Also, in some cases at any rate the errors of measurement must have been minimal, and yet the validity of Burt's equation (p. 58 above) remains unaltered. Moreover, it can easily be shown that if this equation holds good for the correlations as affected by the chance errors, it must also be valid for the true correlations[1].

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[1] See *Ztsch. f. Psychol.* 1907, XLIV. 105–6. Still more convincing evidence is furnished by some work in progress in the laboratory of one of us.

[2] See *Amer. J. of Psychol.* 1904, XV. 264. Also *Ztsch. f. Psychol.* 1907, XLIV. 105.

Since there is no reasonable explanation of the General Factor as spurious—and, moreover, spuriousness is absolutely refuted by the above evidence of 'mental tests'—let us turn to the explanations of it as genuine. The most obvious one is that of 'General Intelligence.' The existence of this power would seem guaranteed by common experience; and even the psychologists who exclude it from their theories seem inclined to revert to it in the hour of practical need. As it would help a person in almost any kind of performance, it would furnish a General Factor, just as required by the correlational evidence. And as it is supposed to be the essence of intellectual activity, it possesses all the dignity that many authors regard as indispensable for such an important central function. In addition, the hypothesis seems to admit of easy empirical verification; for general intelligence is actually being estimated by teachers in the course of ordinary school work, so that their judgments are at once available for use.

But more careful consideration reveals difficulties. To begin with, many critics believe that such estimates, based on haphazard general impressions, are liable to be very erroneous. They point to the low opinion formed by teachers about even the greatest men, such as Newton, Darwin, Spencer, Byron, Dalton, Liebig, v. Humboldt, and innumerable others.

For the purpose of calculating correlation, this erroneousness would be much less fatal if it were wholly of a random character and its approximate amount could be determined. But even for this determination, we need to compare the opinions of two or more teachers who both observe and judge independently. This is rarely feasible in a school. Even when one judge is a teacher and the other is a fellow-pupil, they still to a great extent utilise the same observations, such as published school marks, current anecdotes, etc.; often they go as far as to interchange views[2]. A case is known to one of us where the teachers used to find a satisfactorily high correlation between their respective estimates of their pupils, in spite of these being so rigorously selected as to stand on much the same level of ability. One year, however, they tried the experiment of abstaining from any interchange of observations or opinions about a new lot of pupils. Thereupon, the correlation between the estimates made an extraordinary drop, from about ·80 to about ·30. A repetition of the experiment had the same issue.

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[1] It has been proved that the effect of chance errors on the correlations of any performance is eliminated by multiplying these correlations by a constant (this *Journal*, 1910, III. 275). But the solution of the equation (1) on page 58 is $r_{ap}=AP$, $r_{aq}=AQ$, $r_{bp}=BP$, $r_{bq}=BQ$, where A, B, P, Q are constants, so that the form of this equation is unchanged when all the correlations of any performance are multiplied by any constant.

[2] A former experiment by one of us was peculiarly lucky in this respect. The fellow-pupils had grown up in the same village, and based their estimates mainly on this life-long village experience; whereas the teacher came from elsewhere and only observed the children

An even greater difficulty in dealing with this 'General Intelligence' is that of knowing exactly what it means. Dictionaries give not one but several significations. And no less discordant are the various definitions of investigators of correlations; the latter do not seem to take even their own definitions very seriously, if, indeed, they give any. No one realises this difficulty more than the abler of the teachers themselves; they have frequently protested to us against there being any single manifest 'General Intelligence,' and have declared that their estimates of their pupils would vary largely according to the criterion which they might happen to adopt. This liability to arbitrariness is confirmed by comparison with experimental results. Burt, for instance, found that certain teachers' estimates, instead of exactly coinciding with the General Factor as indicated by the experiments, showed a strong special tendency to base themselves on the power of memorising[1].

On the whole, then, the more these estimates are impartially studied, the more clearly they reveal all the faults that might have been expected from their origin in lay psychology. Far from such estimates ever satisfactorily explaining the experimental data, it is rather the latter which are beginning to elucidate the former. The popular 'General Intelligence' is being resolved into two parts: the one is, indeed, a deep underlying truth, the General Factor; but the other is a superposed mass of obscurity and error[2].

7. *Explanation by 'Intellective Energy.'* For the delicate task of tracing out the precise character of the General Factor we require, naturally enough, finer means than mere popular concepts and casual impressions; we are inevitably thrown back on experiment. Just as

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in and about school (*Amer. J. of Psychol.* 1904, xv. 269). In such exceptional cases, the estimates *may* be nearly independent; but the difficulty remains of knowing when they are so.

[1] This *Journal*, 1909, III. 143.

[2] The above is not in contradiction with the earlier work of one of us. For although the term 'general intelligence' was freely used, this was expressly done as 'denoting an almost *X*' (*Amer. J. of Psychol.* 1905, xvi. 231).

this was the sole effective manner of proving the existence of the General Factor, so also it is the only hopeful way of elucidating its nature. The method to be employed is similar to that necessary in other scientific problems; we have to vary the conditions and examine the effect on the ensuing correlations.

Some of these effects seem not to have much theoretical significance. For instance, when the performers comprising a given 'sample' happen to differ largely from one another in any ability, all the correlations with that ability tend to increase. This is only a natural consequence of the essential nature of correlations; the larger the variation in any component factor, the greater will be its influence on the variation of the whole, that is, the greater will be its power to produce correlation. But the fact is worth remembering, for it explains many differences of experimental results which have wrongly been taken as contradicting one another.

Another influential aspect liable to be overlooked is the 'warming-up' of fore-exercise. The amount given may vary considerably, some investigators preferring little, others much. An attempt has been made to decide on the effect of this difference of procedure, by arguing that the newer the task, the better it must correlate with a person's intelligence. But such loose speculation is not likely to be trustworthy; and experiment shows just the contrary, the effect of the absence of fore-exercise being to decrease this correlation[1]. Nor is the reason far to seek. Without fore-exercise, a person's success largely depends on his recent cognate experiences, which may assist him in varying degrees and sometimes even hinder him[2]; hence, much is left to chance, a fact which must tend to lower any correlation. By fore-exercise, on the other hand, this chance element is in great measure eliminated, leaving success to depend more on the person's innate capacity, or at any rate on his older and more permanent acquirements.

Somewhat different from, and theoretically more important than, the effect of fore-exercise appears to be that of repeating the same test on several occasions. As far as the present evidence goes, the correlations tend to increase only when the performances are difficult for the subject[3]; if they are easy, the correlations tend to decline[4]. As easy activities become on repetition more mechanical and less conscious, we have here some indication that the General Factor looked for is in some way connected with clear awareness, or at any rate opposed to mechanization[1].

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[1] *Amer. J. of Psychol.* 1904, xv. 278. This is one of the reasons why some investigators have obtained lower correlations than others for tests of sensory discrimination.

[2] See Sleight, this *Journal*, 1911, IV. 432.

[3] See Abelson, this *Journal*, 1911, IV. 304. Also Burt's remarkable results with his 'Spot pattern,' *ibid.* 1909, III. 168.

[4] See Burt's other tests, *ibid.*

This inference is supported by Burt's finding the highest correlations to be given by the performances that demand most 'attention' (as understood and estimated by various subjects)[2].

Pointing in the same direction is the fact that the 'higher' operations, such as reasoning, etc., generally produce larger correlations than those essentially dependent on the sensory and motor functions. But this rule is far from being absolute; even the sensory and motor operations appear always to yield a certain amount of correlation, and sometimes a large amount. Moreover, the rule is at once explicable by the greater complexity of these higher processes; for the multitude of independent specific factors must tend to cancel one another, leaving the General Factor more dominant.

The key to the variability of correlation appears to have been furnished by Abelson[3]; he discovered that the amount of correlation yielded by apparently unintellectual performances depends mainly upon the intellectual status of the subjects. In the case of mentally defective children, he found that the most mechanical tasks, for instance, the mere rate of tapping, produced about as much correlation as those involving the higher operations, for instance, the interpretation of pictures[4]. It would appear that at such a low level of ability not even the simplest tasks are acquired thoroughly enough to become mechanical.

Thus the various lines of evidence—the larger correlations usually produced by the operations demanding 'attention,' the reduction of correlation as the performances tend to become mechanical, and the large correlations shown by even the simplest performances of the mentally defective—all these converge, indicating some fundamental opposition between the General Factor, on the one hand, and mechanization by habit, on the other.

But of all the features invariably attending the mechanization of any activity, none is more remarkable than the way in which it *ceases*

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[1] This point is brought out by Abelson. For certain authors, clear awareness is synonymous with 'attention.' But the latter word—at any rate, if used without strict definition—has so many possible meanings that it might almost as well have none.

[2] *Ibid.* 167.

[3] This *Journal*, 1911, IV. 300.

[4] Subsequent (not yet published) work by Abelson has confirmed and extended this discovery.

to interfere with other simultaneous activities. For instance, the first efforts to ride a bicycle are found sufficient to absorb the mind fully; in time, however, the rider becomes able to look freely about him, to ponder over problems, or to light a cigarette. But if the non-mechanized activities are thus distinguished by their acute competition with one another, they must be competing for something; if the enhancement of any one such activity can only occur at the expense of all the others, we can scarcely escape the conclusion that all these manifestations of energy derive—to some extent, at least—from a *general common fund*. In other words, the known facts about the competition among non-mechanized intellectual performances[1] drive us to the conception of a General Common Factor just as inevitably as did our laborious calculations. This happy mutual corroboration of two such different lines of investigation strengthens our confidence in both.

While this conception of a common fund of 'intellective energy,' as we may call it, explains the close connexion between the General Factor and clear awareness, it also suggests that this connexion is only a part of the truth. Quantitative experiment reveals that the same competition extends even to some unconscious processes. For instance, Müller has proved that the process of learning continues long after it ceases to be conscious, and that this unconscious continuance is greatly interfered with by any further mental work, such as learning something else ('retroactive inhibition')[2].

A still more fundamental difference between intellective energy and clear awareness appears probable. The latter, though indispensably requiring the former, may well have further needs also; being one of the characteristics of success in any performance, it is likely to need in addition the specific ability for that performance. A person with specific incapacity for music, whatever may be his intelligence in other respects, appears unable to become clearly aware of melody. Again, on injury to the post-Rolandic convolutions, the patient loses power to 'attend' to his tactile sensations although still quite able to attend to anything else[3]. The very specific character of attention (in the sense of gaining clear awareness) has been further emphasized by Sleight's recent investigation[4].

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[1] The most masterly and exhaustive account of these facts appears to be Wirth's *Experimentelle Analyse der Bewusstseinsphänomene*, 1908.

[2] Müller and Pilzecker, *Ztsch. f. Psychol. Erganzüngsbd.* I.

[3] See the very remarkable investigations of Head, *Brain*, 1912.

[4] This *Journal*, 1911, IV. 435, 439.

On the whole, then, the surprising fact of ability in any performance exhibiting marked correlation with performances of a totally different character seems fundamentally identical with the fact of mental work of any kind inhibiting simultaneous work of totally different kinds. ? Both phenomena appear satisfactorily explained by conceiving that every non-mechanized performance depends partly, indeed, on specific disposition for that performance, but partly on some fund of energy common to all such performances. Those authors are on a false track who are endeavouring to identify the General Factor with any special process of peculiarly high dignity, such as 'intelligence,' 'synthetic power,' etc. On the contrary, the essential nature of the General Factor—and the cause of its hitherto baffling investigation—is precisely the fact that it is disposible for *any* kind of non-mechanized process[1].

8. *Physiological considerations.* So far we have confined ourselves to evidence and considerations of a psychological character. Let us turn briefly to the physiological aspect. We immediately come across a General Factor beyond all dispute, namely, the common blood supply; quantitative and qualitative alterations of this necessarily produce effects upon all the cortical functions. Such alterations are probably mainly responsible for the appreciable fluctuations in the general intellectual level which occur normally even in the same person. A man's ability seems not quite identical in the morning and in the evening, in summer and in winter, after rest and after work, on an empty stomach and on a full one, even standing up and lying down. In the sphere of pathology, the effects are much more marked. Alcohol and other drugs, and the toxins generated in various diseases, produce a universal effect upon all the cerebral functions ranging from diminished ability in the carrying out of every mental performance to states of stupor and coma.

But this explanation, no doubt true as far as it goes, appears to require supplementation. Even if it could be thought adequate to account for the observed correlations, it would be wholly unable to explain the other phenomenon, namely, the acute competition between different simultaneous activities. And just as little can it explain the well known physiological fact that lesions of certain portions of the brain do not produce specific disability of any one function, but rather X. a generalised disability for all performances. Flourens observed that partial removal of the cerebrum of pigeons resulted in no specific defect, but in a general weakness in all directions. The same result was reached in the work of Goltz on dogs. Another case in point is Ferrier's famous labourer, in whom extensive injury of the frontal convolutions produced only general intellectual weakness.

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[1] As regards the explanation of the General Factor, see also the interesting discussion of Wundt (*Grundzüge d. physiol. Psychol.* 6te Aufl. III. 596). Also Burt, this *Journal*, III. 169.

The above physiological observations led in the middle of the last century to the opinion that the cerebrum is functionally equivalent in all its parts. Subsequently, indeed, the work of Broca, Wernicke, Fritsch and Hitzig, and many others brought about the adoption of the contrary view of complete localisation, so that the cerebrum was regarded as a plurality of independent organs. But recent investigation has tended more and more to compromise between these two extreme views; the present evidence indicates that, although some or all of the different cortical regions specifically mediate different functions, nevertheless the efficiency of each region depends vitally upon that of the whole. A physiological explanation which has been advanced and appears to find continually increasing support is that nervous energy admits of 'vicarious' usage. According to this, it may be conceived that every particular mental operation requires two things: firstly, a specific activity of a particular system of neural structures; and secondly, the concurrence of neural energy from the whole, or a large part, of the cortex[1].

Thus the conception of a General Factor, and perhaps even of a common fund of energy, appears as indispensable for physiology as for psychology. Also, there is a complete parallel between, on the one hand, the psychological evidence of the General Factor being chiefly concerned with non-mechanized operations and, on the other hand, the physiological evidence of the solidarity of nervous system being greatest at its highest levels; for instance, sensory or motor lesions in the peripheral nervous system or even in the basal ganglia may easily be strictly local, whereas disturbances of the cortical functions are rarely, if ever, so.

Also, physiology, no less than psychology, indicates the possibility of the General Factor having a more or less composite origin. However

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[1] See McDougall, *Brain*, 1909, xxxii. 253. Also Claparède, *Psychologie de l'Enfant*, 241. In a previous paper by Krueger with one of us, the hypothesis was advanced that in some people the cortex probably has a finer 'plastic function' than in others, that is, a greater power of building up finely differentiated and integrated structures. The present paper deepens this view, indicating that the superior plastic function is merely one aspect or product of the superior intellective energy.

Addendum to footnote on page 72.

This conception of energy is purposely left as undetermined as possible. The present theme would be disturbed by entering into the extremely difficult question, whether this energy should be regarded as physical only or as psychical also. The problem is on a par with that concerning physical and psychical 'dispositions' in memory.

distinct several causes may be, if the effect of each is perfectly general, so too will be the total effect[1].

9. *Importance of the Specific Abilities.* The preceding considerations have shown that every intellectual performance may be regarded as proceeding from two distinct factors: on the one hand, the specific ability or disposition for that particular performance; and on the other general ability, due to the common fund of intellective energy. Hitherto, we have discussed the correlations due to the general ability, almost ignoring the specific abilities, or treating them merely as occasional disturbing elements. This attitude appears justified by the results of correlational investigation up to date, as summarised in Tables II—V. Of the great number of performances entering into these tables, only four pairs have required pooling together; thus the tables present all the data almost exactly as was done by the authors themselves. In spite of this, only three times is the correlation between columns distinctly less than + 1 and never once is it under + ·60. On the whole, the influence of the specific abilities on the correlations has been a vanishing quantity. Small as it has been, it nevertheless requires examining in detail.

We will take first the four cases where pools were made. These were merely due to the similarity of the performances (see p. 59). One was the case of Oehrn described above (p. 59). Another was in the results of Brown, who had used two nearly identical tests of erasing letters. The other two were in the work of Bonser. He had used a pair of tests for each of the following operations, 'mathematical judgment,' 'controlled association,' and 'selective judgment'; he had himself pooled together the pair for mathematical judgment; we treated in the same manner the pairs for the two other operations. That the two tests forming a pair were similar is evidenced by Bonser having classed them together as tests of the same mental operation. The similarity is not, indeed, very marked; but no more is the specific correlation; if the pooling is omitted, the mean correlation between columns falls distinctly short, indeed, of + 1, but still remains positive and high.

Next, as regards the three cases where the correlation falls appreciably below positive unity[2], one of these will be treated presently.

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[1] This possible compositeness has been noted from the first, see *Amer. J. of Psychol.* 1904, xv. 273, 284.

[2] In selecting these three as having 'appreciable' deviations from + 1, we have had to trust to our general impression. Later on, we hope to work out the probable error involved.

The other two come from Burt; they are due to both schools showing, as said above, specific correlation between the teachers' estimates of intelligence and the experimental tests of memory. If the columns for the estimates of intelligence are omitted, the values in Table II rise from + ·67 to + ·98 for the elementary school and from + ·63 to + ·98 for the High Grade school.

In addition to these seven cases of appreciable specific correlation in the material used for our tables, there appears one more case in some material that could not be utilised in this way; this was the series of Thorndike, Lay, and Dean (see footnote 3 to p. 61). These investigators had found the correlation between two different tests of sensory discrimination (weight and length) much too high to be solely attributable to their common dependence on general intelligence as estimated by their teachers and fellow pupils. This excess of correlation was thereupon ascribed by them to both performances being on the 'sensory level[1]' and was made the basis of energetic controversy. But herein they overlooked, among other things, the fact that both tests were done by the method of 'reproduction,' which is now generally discredited on account of leaving to the subjects a wide arbitrary choice of procedure. Those subjects whose procedure happened to be most elaborate or otherwise most advantageous would naturally score in both tests; hence would arise a disturbing specific correlation between them just as actually observed. Nevertheless, the work of these authors appears valuable; for it reminds us that the similarity between two performances is by no means confined to the aspect expressed in the current names of mental operations, such as sensory discrimination, memory, reasoning, movement, etc.; all the other aspects, such as the psychophysical method, demand consideration also, and often may be much more important.

But surprisingly small as has been the rôle of specific abilities in all previous experimental correlations, there are reasons for questioning whether this is the case in ordinary life also. In the latter, even a very specialised performance may have ultimate consequences of considerable magnitude. An 'ear' for melody is known to be particularly specific, that is, independent of other elementary capacities; and yet it forms the indispensable gateway to the world of music. If the common diagnosis of 'word blindness' in backward children is correct, the power of associating visual forms with meanings stands very isolated; and yet it constitutes the sole approach to normal scholastic instruction. Possibly the power to hold a rifle accurately may be very specific, and yet form a factor in the supremacy of nations.

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[1] In fact, this case seems to be the sole published evidence in support of the 'level.'

Again, the narrowness of range of specific abilities may not obtain universally; there may be important exceptions. For instance, the specific correlation discovered by Burt between school work and tests of memory looks as if here the range might cover all the diverse activities coming under the head of memorisation. This deduction is corroborated by having already been reached on quite different and exclusively experimental grounds[1].

Another important point is that, while the theory of a General Factor was originally applied only to the total efficiency of performances, it is often desirable to analyse this efficiency into various aspects, for instance, to consider the accuracy and speed separately. Brown, in particular, measured many performances from different aspects, but drew up his table of coefficients treating quite alike the correlations between total efficiencies, between part efficiencies of the same performance, between part efficiencies of different performances, and between part and total efficiencies. It is astonishing that, in spite of this, the General Factor continued even here to show itself almost exclusively effective in producing correlation. But in the single instance where the mean correlation between columns fell appreciably below + 1 (*i.e.* to +·73), it is clearly traceable to this treatment of part efficiencies. The correlations indicate that those children who preferred speed to accuracy in one performance tended slightly to make the same preference in other performances also. A similar case occurs with Bonser, whose 'spelling' was not an independent performance, but was merely estimated from the papers done in the test of 'easy opposites.' On looking into the correlations, it appears that those children who attend much to their spelling were slightly handicapped in evoking the 'easy opposites'; this negative correlation was superposed on, and slightly detracted from, the basal positive correlation between them due to the General Factor. The disturbance was so small as practically to disappear when 'easy opposites' was pooled with the other test of controlled association. But some work of the present authors (shortly to be published) indicates that such specific correlation between part efficiencies can be very much larger.

Further specific correlation may occur in a more or less accidental manner. For instance, proficiency in natural science and proficiency in mathematics are likely to go specially together, merely because the same children generally take up both studies[1]. Again, performances may be linked together by the errors in the measurements of the one being correlated with those in the measurements of the other. Innumerable other possibilities of special relation will readily suggest themselves on reflection.

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[1] By Krueger with one of us, *Ztsch. f. Psychol.* 1907, XLIV. 103.

The present paper, then, is far from denying that specific abilities may play a large rôle. Our object is not to oppose Thorndike's invaluable work on them, but to further it[2]. We hope to have done so in showing that their correlations do not occur in a pure state, but only superposed upon correlation of a more general character. Their investigation is a much more delicate matter than hitherto supposed. A new technique is required.

10. *Applications of the doctrine of General and Specific Abilities.* When this doctrine was first advanced, it was done with an acknowledgment "that it must acquire a much vaster corroborative basis before we can accept it even as a general principle and apart from its inevitable eventual corrections and limitations[3]." The corroborative basis then demanded would seem to be now supplied by the array of facts quoted in the present paper. It is therefore legitimate to indicate briefly that this doctrine has far-reaching bearings on psychological theory and practice.

In the first place, radical modifications appear needed in the current modes of interpreting correlations. These are suffering from an almost universal confusion between general and specific ability. The point may be illustrated by a correlation of ·49 recently found between the power of interpreting pictures and that of memorising sentences[4]. Almost every psychologist would at once conclude that this ·49 represented the extent to which the process of interpreting pictures involved the exercise of memory. But let us take any other two of the tests used on that occasion, let us say, the rate of tapping and the discrimination of length. Let the four performances be indicated by *p, s, t,* and *l*, so that the ·49 may be denoted by r_{ps} and the other intercorrelations similarly. If equation (1) on p. 58 holds good, then

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[1] See *Ztsch. f. Psychol.* 1907, XLIV. 87.

[2] Still less is it in opposition to his work on 'formal training,' with which, in the main, we cordially agree (see Sleight's paper in this *Journal*, 1911, IV. 386). Variations of training, within normal limits, appears to have no appreciable influence on the General Factor, but only on the specific ones. This will be readily understood from our explanation of their respective natures.

[3] *Amer. J. of Psychol.* 1904, XV. 273.

[4] Abelson, this *Journal*, 1911, IV. 294. It should be added that this author did not make the confusion in question.

$$r_{ps} = \frac{r_{pt} \times r_{ts}}{r_{tt}}.$$

And in actual fact, this equation was closely verified, since $r_{pt} \times r_{ts} \div r_{tt}$ came to $\cdot 61 \times \cdot 32 \div \cdot 37 = \cdot 53$. Thus, on the doctrine demonstrated in this paper, r_{ps} is wholly determined by three coefficients r_{pt}, r_{ts}, r_{tt}, all of which are quite unconcerned with the extent to which interpreting pictures involves memory. And r_{ps} might equally well be determined from any other appropriate set of three coefficients. It is no indication at all of any specific relation between interpreting pictures and remembering, but of the mean degree in which these two performances depend respectively on general ability.

But the effect of the doctrine is by no means confined to correlational coefficients. Take, for instance, the numerous experiments in various countries designed to ascertain how memory depends upon age. The elaborate statistics obtained are really, it seems hardly questionable, governed principally by the growth and fluctuations of the children's general rather than specific ability. The endeavour to discover which epoch of life is specially adapted to memory has gone completely astray. The same confusion arises equally, of course, when statistics are replaced by casual impressions or any other inexact method; for instance, when a psychiatrist diagnoses 'loss of memory' where really there is a depreciation of general ability. These illustrations are taken from memory, as it at present dominates experimental investigation; but our considerations apply equally well to any other sort of intellectual activity.

The above chiefly concerns what is sometimes known as 'individual psychology' and is held to be a rather isolated branch of inquiry. But there are signs of a change of opinion. Many of us are looking forward to the day when psychological textbooks will become less exclusively preoccupied with sensations and epistemology, but turn rather to appreciating the mind or 'soul' as the agent in conduct[1]. If this hope is realised, the General Factor, or common fund of intellective energy,—with its intimate bearings on every kind of intellectual process and, above all, on 'attention'—seems bound to take a prominent place.

Larger still than the above applications to psychological theory are those to psychological practice. Indeed, so many possibilities suggest themselves, that it is difficult to speak freely without seeming extravagant. The application to psychiatry will be discussed in a separate paper, which we hope to publish shortly. Another obvious one is to relieve the overburdened institution of examinations. These have various natural purposes—the ascertainment whether schools are being conducted adequately, whether pupils are working diligently, whether they are acquiring the instruction needed for particular professions—which are admittedly incompatible with the further function of measuring individual differences of ability. But, as we have seen in this paper, the backbone of this ability, the General Factor, the intellective energy, can be disentangled from all such irrelevant matters and submitted to precise experimental determination on its own account. This determination is becoming so easy, that it might well be carried out regularly. It seems even possible to anticipate the day when there will be yearly official registration of the 'intellective index,' as we will call it, of every child throughout the kingdom[1].

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[1] Significant of the coming change are such definitions of psychology as that of McDougall (*Physiological Psychology*, p. 1); also such books as *Le Leggi del Lavoro Mentale* by Guido della Valle, or the works of C. Henry (Paris).

This registration, controlled and digested by an expert bureau, could scarcely fail to shed a flood of light on many vital problems. We should learn how the intellective index depends on age, sex, nutrition, physical exercise, fatigue, climate, formal training, etc. Even the influence of heredity would become much more accessible to study. And this clearing up of our knowledge of general ability would be the first and decisive step towards investigating the specific abilities also, or any further factor at present obscured by all these. We should even be helped in the study of the other great side of the mental make-up, the emotions, impulses, and volitions.

In course of time, there seems no reason why the intellective index (or system of indices) should not become so well understood, as to enable every child's education to be properly graded according to his or her capacity. Thus the present difficulties of picking out the abler children for more advanced education and the 'mentally defective' children for less advanced would vanish in the solution of the more general problem of adapting education to all.

Still wider—though doubtless, dimmer—are the vistas opened up as to the possible consequences in adult life. It seems not altogether chimaeric to look forward to the time when citizens, instead of choosing their career at almost blind hazard, will undertake just the professions really suited to their capacities. One can even conceive the establishment of a minimum index to qualify for parliamentary vote, and, above all, for the right to have offspring.

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[1] It has already been determined, although naturally still in a very crude manner, for the entire school population of one small city, about 2000 children (see *Ped. Sem.* June, 1911).

14. *Conclusions.*

(1) At present, there exists such a great divergence of opinion about the correlation between different intellectual performances, as to impede gravely the progress of psychology.

(2) But closer consideration of all the actual data of the different authors shows that this divergence is merely due to gross misinterpretation. In reality, all the facts indicate unanimously, that the correlation arises through all the performances, however different, depending partly on a General Common Factor.

(3) The same conclusion is indicated by the recent surprising regeneration of 'mental tests.' For this appears to be due to both their purpose and method having been transformed in accordance with the theory of a General Factor.

(4) This General Factor is not any special sort of process, such as an 'intelligent' or 'synthetic' operation.

(5) The fact of correlation existing between quite different intellectual performances seems to be fundamentally identical with the fact that any such performance inhibits quite different simultaneous ones. Both phenomena are explicable by conceiving that every performance depends partly on some *common fund of energy.* This, then, is the required General Factor. Explanation by 'attention' seems inadequate.

(6) Every performance depends, not only on this General Factor, but also in varying degree on a factor specific to itself and all very similar performances.

(7) This doctrine of general and specific factors (or, viewed from another aspect, general and specific abilities), though based primarily on psychological evidence, appears to be no less indispensable for understanding the facts of physiology. Every intellectual act appears to involve, both the specific activity of a particular system of cortical neurons, and also the general energy of the whole cortex.

(8) The doctrine appears to have wide bearings on general psychological theory and practice.

APPENDIX.

I. To show that, when the proportional equation holds good, the correlation between pairs of columns (or rows) in a table of coefficients becomes $= 1$.

The proportional equation (given on p. 58) is

$$\frac{r_{ap}}{r_{aq}} = \frac{r_{bp}}{r_{bq}} \quad \text{..(1)},$$

and the solution of this equation is

$$r_{ap} = AP, \quad r_{bp} = BP, \quad r_{aq} = AQ, \quad r_{bq} = BQ \quad \text{......................(2)},$$

where A is constant for all correlations with the performance a, and similarly B, P, Q.

Now, let $R_{ab} \equiv$ the correlation between the column for a and that for b, so that

$$R_{ab} = \frac{S(r_{ax} - \bar{r}_{ax})(r_{bx} - \bar{r}_{bx})}{\sqrt{S(r_{ax} - \bar{r}_{ax})^2 . S(r_{bx} - \bar{r}_{bx})^2}},$$

where scoring denotes the arithmetical mean for all values of x, and by (2)

$$= \frac{S(AX - A\bar{X})(BX - B\bar{X})}{\sqrt{S(AX - A\bar{X})^2 . S(BX - B\bar{X})^2}} = 1 \quad \text{......................(3)}.$$

While if the r's are independent of one another, clearly $R_{ab} = 0$.

II. Correction of R_{ab} for errors of sampling in the r's on which it is based.

Indicating correct values (*i.e.* values free from sampling error) by dashes,

$$R'_{ab} = \frac{S(\rho'_{xa} \rho'_{xb})}{\sqrt{S(\rho'^2_{xa}) . S(\rho'^2_{xb})}},$$

where ρ'_{xa} denotes $r'_{xa} - \bar{r}'_{xa}$, and similarly ρ'_{xb}. The term on the right may easily be expanded to

$$\frac{S(\rho_{xa}\rho_{xb} - \epsilon_{xa}\epsilon_{xb} - \rho'_{xb}\epsilon_{xa} - \rho'_{xa}\epsilon_{xb})}{\sqrt{S(\rho^2_{xa} - \epsilon^2_{xa} - 2\rho'_{xa}\epsilon_{xa}) . S(\rho^2_{xb} - \epsilon^2_{xb} - 2\rho'_{xb}\epsilon_{xb})}},$$

where ρ denotes the value of ρ' for any particular observed sample and ϵ is its sampling error, so that $\rho = \rho' + \epsilon$. This may be further expanded to

$$\frac{S(\rho_{xa}\rho_{xb}) - S(\epsilon_{xa}\epsilon_{xb}) - \frac{1}{n} S(\rho'_{xb}) . S(\epsilon_{xa}) - \frac{1}{n} S(\rho'_{xa}) . S(\epsilon_{xb})}{\sqrt{S(\rho^2_{xa}) - S(\epsilon^2_{xa}) - \frac{2}{n} S(\rho'_{xa}) . S(\epsilon_{xa})} \sqrt{S(\rho^2_{xb}) - S(\epsilon^2_{xb}) - \frac{2}{n} S(\rho'_{xb}) . S(\epsilon_{xb})}},$$

approximately, since ρ' will not in general be correlated with ϵ,

$$= \frac{[S(\rho_{xa}\rho_{xb}) - \Sigma(\epsilon_{xa}\epsilon_{xb})] - \left[D(\epsilon_{xa}\epsilon_{xb}) + \frac{1}{n} S(\rho'_{xb}) . S(\epsilon_{xa}) + \frac{1}{n} S(\rho'_{xa}) . S(\epsilon_{xb})\right]}{\sqrt{[S(\rho^2_{xa}) - \Sigma(\epsilon^2_{xa})] - \left[D(\epsilon^2_{xa}) + \frac{2}{n} S(\rho'_{xa}) . S(\epsilon_{xa})\right]} \sqrt{[S(\rho^2_{xb}) - \Sigma(\epsilon^2_{xb})] - \left[D(\epsilon^2_{xb}) + \frac{2}{n} S(\rho'_{xb}) . S(\epsilon_{xb})\right]}},$$

where Σ denotes the mean value of S for a very large number of randomly chosen samples of the size in question, and $D = S - \Sigma$. Then, representing each above expression in square brackets by a single letter, we may write the above expression as

$$\frac{S_1 - \Delta_1}{\sqrt{S_2 - \Delta_2}\sqrt{S_3 - \Delta_3}}$$

$$= \frac{S_1}{\sqrt{S_2 S_3}}\left(1 - \frac{\Delta_1}{S_1} + \frac{\Delta_2}{2S_2} + \frac{\Delta_3}{2S_3} \pm \text{higher powers of } \frac{\Delta}{S}\right) \quad \ldots\ldots\ldots\ldots\ldots(4).$$

And if the Δ's are small compared with the corresponding S's, *i.e.* if the ϵ's are small compared with their ρ's, we get approximately, on taking a mean of different samples,

$$R'_{ab} = \frac{S_1}{\sqrt{S_2 S_3}} = \frac{S(\rho_{xa}\rho_{xb}) - \Sigma(\epsilon_{xa}\epsilon_{xb})}{\sqrt{S(\rho^2_{xa}) - \Sigma(\epsilon^2_{xa})}\sqrt{S(\rho^2_{xb}) - \Sigma(\epsilon^2_{xb})}} \quad \ldots\ldots\ldots\ldots(5).$$

It is therefore only required to find the Σ's.

Now, $\epsilon = \rho' - \rho = e - \bar{e}$, where $e = r' - r$. Hence it is readily found that

$$S(\epsilon_{xa}\epsilon_{xb}) = S(e_{xa}e_{xb}) - \frac{1}{n}S(e_{xa}) \,.\, S(e_{xb})$$

$$= \frac{n-1}{n}S(e_{xa}e_{xb}) - \frac{1}{n}\underset{x\ y}{SS}(e_{xa}e_{yb}),$$

where x and y are always different.

Hence $$\Sigma(\epsilon_{xa}\epsilon_{xb}) = \frac{n-1}{n} S\left[\frac{1}{N}T(e_{xa}e_{xb})\right] - \frac{1}{n}\underset{x\ y}{SS}\left[\frac{1}{N}T(e_{xa}e_{yb})\right],$$

where T denotes the total sum for all the samples, and N is the number of samples,

$$= \frac{n-1}{n} S(R_{xa-xb}\sigma_{xa}\sigma_{xb}) - \frac{1}{n}\underset{x\ y}{SS}(R_{xa-yb}\sigma_{xa}\sigma_{xb}),$$

where $\sigma_{xa} \equiv$ the root mean square sampling deviation of r_{xa}, and similarly σ_{xb}, while $R_{xa-xb} \equiv$ the correlation between e_{xa} and e_{xb} for different samples, and similarly R_{xa-yb},

$$= \frac{n-1}{n^2} S(R_{xa-xb}) \,.\, S(\sigma_{xa}\sigma_{xb}) - \frac{1}{n^2(n-1)}\underset{x\ y}{SS}(R_{xa-yb})\underset{x\ y}{SS}(\sigma_{xa}\sigma_{yb}) + D, \text{ say ;}$$

and, if we may neglect D,

$$= (n-1)\,\overline{\sigma_{xa}\sigma_{xb}}\,(\bar{R}_{xa-xb} - \bar{R}_{xa-yb}) \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(6).$$

From (5) and (6) we get finally

$$R'_{ab} = \frac{S(\rho^2_{xa}) - (n-1)\,\overline{\sigma_{xa}\sigma_{xb}}\,(\bar{R}_{xa-xb} - \bar{R}_{xa-yb})}{\sqrt{S(\rho^2_{xa}) - (n-1)\,\overline{\sigma^2_{xa}}\,(1 - \bar{R}_{xa-ya})}\sqrt{S(\rho^2_{xb}) - (n-1)\,\overline{\sigma^2_{xb}}\,(1 - \bar{R}_{xb-yb})}} \ldots(7).$$

Here, σ_{xa} is known to be = prob. error of r_{xa} divided by ·6745, and similarly σ_{xb}. The $\bar{R}$'s are given by the equation

$$\bar{R}_{st-uv} = \left[\begin{array}{l} \quad (r_{su} - r_{st}r_{tu})(r_{tv} - r_{tu}r_{uv}) \\ + (r_{sv} - r_{uv}r_{su})(r_{tu} - r_{st}r_{su}) \\ + (r_{su} - r_{sv}r_{uv})(r_{tv} - r_{st}r_{uv}) \\ + (r_{sv} - r_{tv}r_{st})(r_{tu} - r_{tv}r_{uv}) \end{array}\right] \div 2(1 - r^2_{st})(1 - r^2_{uv})^*,$$

where s, t, u and v are any values.

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* See Pearson and Filon, *Phil. Trans. Roy. Soc. Lond.* A. CXCI. 262.

The full calculations of the $\bar{R}$'s involve a number of terms that are tedious to work out and usually insignificant in amount. We have found that, on the whole, a very approximate value is given by

$$\mathrm{R}'_{ab} = \frac{\mathrm{S}\,(\rho_{xa}\rho_{xb}) - (n-1)\,r_{ab}\,\overline{\sigma_{xa}\sigma_{xb}}}{\sqrt{\mathrm{S}\,(\rho^2_{xa}) - (n-1)\,\overline{\sigma^2_{xa}}}\,\sqrt{\mathrm{S}\,(\rho^2_{xb}) - (n-1)\,\overline{\sigma^2_{xb}}}} \;\dots\dots\dots\dots(8),$$

where σ_{xa} denotes, as before, the prob. error of r_{xa} divided by ·6745, and similarly σ_{xb}, while the scoring indicates the mean for all values of x.

We took the median raw correlation entering into each of the values given in Table II, and corrected it both by the full (7) and by the above approximate method (8). The median discrepancy came to only ·02. It tended equally to be positive and negative, so that the mean result for the two methods was identical to the first two decimal places. We have, therefore, adopted this approximate method.

III. To show that the degree of validity of the proportional equation $\frac{r_{ap}}{r_{aq}} = \frac{r_{bp}}{r_{bq}}$ is measured by the coefficient

$$\frac{S\,(r_{xa}r_{xb}) - \frac{1}{n}\,S\,(r_{xa})\,.\,S\,(r_{xb})}{\pm\sqrt{S\,(r^2_{xa})\,.\,S\,(r^2_{xb})} - \frac{1}{n}\,S\,(r_{xa})\,.\,S\,(r_{xb})},$$

where the sign to be prefixed to the root is that of $S\,(r_{xa}r_{xb})$*.

From the proportional equation, it is evident that $r_{xb} = r_{xa}k$, where k is constant for all values of x. Hence $S\,(r^2_{xb}) = k^2 S\,(r^2_{xa})$ and $k = \pm\sqrt{\frac{S\,(r^2_{xb})}{S\,(r^2_{xa})}}$, the sign of k being, clearly, that of $S\,(r_{xa}r_{xb})$.

Now, it is manifest that the above coefficient, say C_{ab}, varies directly as $\pm\frac{S\,(r_{xa}r_{xb})}{\sqrt{S\,(r^2_{xa})\,.\,S\,(r^2_{xb})}}$, where the sign to be prefixed is that of $S\,(r_{xa}r_{xb})$. Hence, subtracting unity and multiplying by $-2k$ times the denominator, C_{ab} varies inversely as

$$2k\sqrt{S\,(r^2_{xa})\,.\,S\,(r^2_{xb})} - 2k\,S\,(r_{xa}r_{xb}),$$

or as

$$k^2 S\,(r^2_{xa}) + S\,(r^2_{xb}) - 2k\,S\,(r_{xa}r_{xb}),$$

or as

$$S\,(r_{xa}k - r_{xb})^2.$$

Moreover, it has the convenience of varying from unity, when the proportional equation is completely valid, down to zero, where the r's are quite uncorrelated with one another.

Evidently, we might equally well take k as $= \frac{S\,(r_{xb})}{S\,(r_{xa})}$. In this case, it could easily be shown that we could get the coefficient

$$\frac{2S\,(r_{xa}r_{xb}) - \frac{2}{n}\,S\,(r_{xa})\,.\,S\,(r_{xb})}{kS\,(r^2_{xa}) + \frac{1}{k}\,S\,(r^2_{xb}) - \frac{2}{n}\,S\,(r_{xa})\,.\,S\,(r_{xb})}.$$

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* Throughout the present investigation the sign is +.

But this coefficient has the disadvantage of a larger sampling error; it becomes quite illusory for this reason whenever $S(r_{xa})$ or $S(r_{xb})$ approaches zero.

IV. Correction of C_{ab} for errors of sampling in the r's upon which it is based.

$$C'_{ab}=\frac{S(r'_{xa}r'_{xb})-\frac{1}{n}S(r'_{xa}).S(r'_{xb})}{\sqrt{S(r'^2_{xa}).S(r'^2_{xb})}-\frac{1}{n}S(r'_{xa}).S(r'_{xb})},$$

which, by reasoning similar to that in II, becomes

$$=\frac{\left[S(r_{xa}r_{xb})-\Sigma(e_{xa}e_{xb})+\frac{1}{n}S(e_{xa}).S(e_{xb})-\frac{1}{n}S(r_{xa}).S(r_{xb})\right]+\frac{1}{n}[S(r'_{xa}).S(e_{xb})+S(r'_{xb}).S(e_{xa})]}{\begin{array}{r}\sqrt{[S(r^2_{xa})-\Sigma(e^2_{xa})]-\left[D(e^2_{xa})+\frac{2}{n}S(r'_{xa}).S(e_{xa})\right]}\\ \sqrt{[S(r^2_{xb})-\Sigma(e^2_{xa})]-\left[D(e^2_{xa})+\frac{2}{n}S(r'_{xa}).S(e_{xa})\right]}\\ -\frac{1}{n}[S(e_{xa}).S(e_{xb})-S(r_{xa}).S(r_{xb})]+\frac{1}{n}[S(r'_{xa}).S(e_{xb})+S(r'_{xb}).S(e_{xa})]\end{array}},$$

which, as before, may be written as

$$\frac{S_4-\Delta_4}{\sqrt{S_5-\Delta_5}\sqrt{S_6-\Delta_6}-(S_7-\Delta_7)}$$

$$=\frac{S_4}{\sqrt{S_5.S_6}-S_7}$$ approximately on a mean, when the e's are small compared with the r's,

$$=\frac{S(r_{xa}r_{xb})-S(e_{xa}e_{xb})+\frac{1}{n}S(e_{xa}).S(e_{xb})-\frac{1}{n}S(r_{xa}).S(r_{xb})}{\sqrt{S(r^2_{xa})-\Sigma(e^2_{xa})}\sqrt{S(r^2_{xb})-\Sigma(e^2_{xb})}+\frac{1}{n}S(e_{xa}).S(e_{xb})-\frac{1}{n}S(r_{xa}).S(r_{xb})}$$

$$=\frac{S(r_{xa}r_{xb})-(n-1)\overline{\sigma_{xa}\sigma_{xb}}(\bar{R}_{xa-xb}-\bar{R}_{xa-yb})-\frac{1}{n}S(r_{xa}).S(r_{xb})}{\sqrt{S(r^2_{xa})-n\overline{\sigma^2_{xa}}}\sqrt{S(r^2_{xb})-n\overline{\sigma^2_{xb}}}+\overline{\sigma_{xa}\sigma_{xb}}[1-(n-1)\bar{R}_{xa-yb}]-\frac{1}{n}S(r_{xa}).S(r_{xb})} \quad \ldots(9),$$

where σ and $\bar{R}$ have the same meanings as in (7).

Here again, a fair average approximation similar to that in (8) may be got by

$$\mathbf{C'_{ab}}=\frac{\mathbf{S(r_{xa}r_{xb})-(n-1)\,r_{ab}\overline{\sigma_{xa}\sigma_{xb}}-\frac{1}{n}S(r_{xa}).S(r_{xb})}}{\mathbf{\sqrt{S(r^2_{xa})-(n-1)\overline{\sigma^2_{xa}}}\sqrt{S(r^2_{xb})-(n-1)\overline{\sigma^2_{xb}}}-\frac{1}{n}S(r_{xa}).S(r_{xb})}} \quad \ldots\mathbf{(10)},$$

which has been used in the preceding investigation.

(It may be recalled here, that σ_{xa} denotes the prob. error of r_{xa} divided by ·6745, and similarly σ_{xb}. The scoring indicates the mean for all values of x.)

V. To show that, if the multifocal theory holds good, the mean correlation between columns will, in general, be negative.

Let $P_1, P_2, \ldots, P_{n_p}, Q_1, Q_2, \ldots, Q_{n_q}, \ldots$ denote the various performances, and suppose the sole non-random character of the correlations between them to be that these correlations are high between performances denoted by the same letter (*e.g.* between P_1 and P_2), but low between performances denoted by different letters (*e.g.* between P_1 and Q_1).

Take first the correlation between the column for one of the P's and that for one of the Q's.

Let p_h, p_l, q_h and q_l denote high and low coefficients in the two columns respectively. Let $n_p+n_q+\ldots$ be denoted by n. Then, summing the products of the type $p_h q_l$ in the numerator of R_{pq}, we get

$$\Sigma(p_h q_l)=(n_p-1)\left\{\frac{(n_p-1)[p_h]+(n-n_p-1)[p_l]}{n-2}-[p_h]\right\}$$
$$\left\{\frac{(n_q-1)[q_h]+(n-n_q-1)[q_l]}{n-2}-[q_l]\right\} \quad \ldots\ldots(1),$$

where $(n_p-1)[p_h]$ denotes the sum of n_p-1 values of the type p_h, only differing from one another randomly; the other square brackets have a similar meaning. Summing similarly the other terms entering into R_{pq}, we get eventually

$$R_{pq}=-\frac{(n_p-1)(n_q-1)(n-2)[(p_h-p_l)(q_h-q_l)]}{(n-2)\sqrt{(n_p-1)(n-n_p-1)[p_h-p_l]^2}\sqrt{(n_q-1)(n-n_q-1)[q_h-q_l]^2}} \ldots(2),$$

which reduces to

$$-\frac{\sqrt{n_p-1}\sqrt{n_q-1}}{\sqrt{n-n_p-1}\sqrt{n-n_q-1}}\cdot\frac{1}{\sqrt{(1+d_p^2)(1+d_q^2)}} \quad \ldots\ldots\ldots\ldots\ldots\ldots(3),$$

where d_p^2 denotes $\dfrac{\overline{(p_h-p_l)^2}-(\overline{p_h-p_l})^2}{(\overline{p_h-p_l})^2}$, scores indicating means as before.

It may be found in a similar manner that

$$R_{pp}=\frac{1}{1+d_p^2},\quad R_{qq}=\frac{1}{1+d_q^2} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(4).$$

But the sum of the R's for all the pairs in the table

$$=\sum_{pp}(R_{pp})+\sum_{qq}(R_{qq})+\ldots+\sum_{pq}(R_{pq})+\ldots \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots(5).$$

The expression on the right may be split up, so as to furnish for every different pair of letters, *e.g.* P and Q, a value which we will call V_{pq}; this, utilising (3) and (4), may easily be found to be

$$\frac{n_p n_q(n_p-1)}{2(n-n_p)}\cdot\frac{1}{1+d_p^2}+\frac{n_p n_q(n_q-1)}{2(n-n_q)}\cdot\frac{1}{1+d_q^2}$$
$$-\frac{n_p n_q\sqrt{n_p-1}\sqrt{n_q-1}}{\sqrt{n-n_p-1}\sqrt{n-n_q-1}}\cdot\frac{1}{\sqrt{1+d_p^2}\sqrt{1+d_q^2}}.$$

Hence, if n_p is not very different from n_q, nor d_p from d_q, we get approximately

$$V_{pq}=\frac{n_p n_q\sqrt{n_p-1}\sqrt{n_q-1}}{\sqrt{1+d_p^2}\sqrt{1+d_q^2}}\left\{\frac{1}{\sqrt{n-n_p}\sqrt{n-n_q}}-\frac{1}{\sqrt{n-n_p-1}\sqrt{n-n_q-1}}\right\}\ldots(6),$$

which is obviously a minus quantity; and therefore the mean of the R's must be so also.

(*Manuscript received November* 1*st*, 1911.)

PUBLICATIONS RECENTLY RECEIVED.

Das Gedächtniss; die Ergebnisse der experimentelle Psychologie und ihre Anwendung in Unterricht und Erziehung. Von Professor Dr Max Offner. Zweite, vermehrte Auflage. S. xi + 258. Berlin: Reuter und Reichard, 1911. Mk 3.50; geb., Mk 4.50.

That a second edition of this book has been published only two years after the appearance of the first is sure enough sign of its utility in bringing home to educationists the value and bearing of past experimental work on memory. No other monograph deals in so comprehensive and thorough a way with this subject. It is not a mere compendium of experimental work, but deals in a systematic way with the general psychology of memory, and with such special topics as the forms of readiness, adequate and inadequate incitement (*Anregung*) of dispositions, mediate association and reproduction, reproduction through contrast, etc. The author believes that in this edition he has simplified the exposition of his views, so far as is possible without the sacrifice of accuracy.

William James. By Emile Boutroux, translated from the second edition by A. and B. Henderson. Pp. vii + 126. London: Longmans, 1912. 3*s.* 6*d.* net.

An excellent translation of Professor Boutroux's sympathetic study of the life, personality and philosophical, pedagogical and psychological writings of William James.

Animal Intelligence: experimental studies. By Professor Edward L. Thorndike. The Animal Behavior Series. Pp. viii + 297. New York: The Macmillan Co., 1911. 7*s.* net.

"The main purpose of this volume," as stated in the preface, "is to make accessible to students of psychology and biology the author's experimental studies of animal intellect and behavior...To the reports of experimental studies are added two new essays dealing with the general laws of human and animal learning." Certain of Thorndike's papers, which form a landmark in the progress of animal psychology, have long been out of print. Their re-publication and the additional two chapters on Behaviour and on the Evolution of the Human Intellect serve a most valuable purpose.

Vorlesungen zur Einführung in die experimentelle Pädogogik und ihre psychologischen Grundlagen. Von Professor Dr Ernst Meumann. Zweite, umgearbeite und vermehrte Auflage. Erster Band. S. xix + 725. Leipzig: Engelmann, 1911.

The first edition of this well-known work, which appeared in 1907, was exhausted two years later. The new edition, which is to be completed in three volumes, may in many respects be considered as a new work, so complete and drastic has been the author's revision. The present volume is devoted to the development of the child; it includes chapters on the experimental investigation of sensation, perception, apperception, memory, thought, language, speech, imagination, suggestibility, feeling, will, interests and attention, in their genetic aspects; it ends with a tentative formulation of the general 'laws' of mental development. Increased space is found in this edition for descriptions of the methods and *technique* of pedagogical experiments. Their application is now extended from elementary school subjects to the higher education of adolescents.

Motive Force and Motivation-Tracks: a research in will psychology. By E. Boyd Barrett, S. J. Pp. xiv + 225. London: Longmans, 1911. 6*s.* net.

This is one of a series of important experimental investigations on the psychology of will, carried out under the direction of Professor Michotte at Louvain University. The main method of experiment was as follows. Eight liquids, differing in degree of agreeableness to the taste, were prepared, and to each a monosyllabic nonsense name was attached. The subjects, five in number, thoroughly learnt to associate these names with the respective tastes. Then the choice experiments began, the subjects previously receiving the instruction—"Two words will appear corresponding to the substances in the two glasses. You are to choose between the substances and to drink at once the liquid which you have chosen." Four stages of the choice act are distinguished by the writer: the perception of the words, the motivation, the choice, and the realisation of the choice. He considers that the motive forces of the alternative chosen and of the alternative rejected are reflected in the speed of the choice act (chronoscopically determined). By motivation-tracks he means the tendencies of the individual to act in a definite characteristic manner under similar conditions. The concluding chapters of the monograph deal with the evolution of motivation, automatism, hesitation, hedonism, the relativity of values and the psychology of character.

Die differentielle Psychologie in ihren methodischen Grundlagen. Von Professor Dr William Stern. S. ix + 503. Leipzig: Barth, 1911. Mk 11; geb., Mk 12.

This is an entirely new work written by the author in place of a second edition of his well-known book, *Über Psychologie der individuellen Differenzen,* which was published in 1900. It is new both in arrangement and in subject matter. The first of the four sections into which it is divided deals with general methods of experiment, the second with variation and correlation and the third with the investigation of 'individualities'; the fourth section contains a bibliography of over 1500 contributions on the subject. There are also appendices on the classification of the temperaments, on mental tests, etc.

La théorie du rhythme et le rhythme du français déclamé, avec une étude "expérimentale" de la déclamation de plusieurs poètes et comédiens célèbres, du rhythme des vers italiens, et des nuances de la durée dans la musique. Par Eugène Landry. Pp. 427. Paris: Champion, 1911.

Religion and Modern Psychology. By J. Arthur Hill. Pp. 200. London: Rider and Son, 1911. 3*s.* 6*d.* net.

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'PSYCHICAL DISTANCE' AS A FACTOR IN ART AND AN AESTHETIC PRINCIPLE.

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I.

1. THE conception of 'Distance' suggests, in connexion with Art, certain trains of thought by no means devoid of interest or of speculative importance. Perhaps the most obvious suggestion is that of *actual spatial* distance, i.e. the distance of a work of Art from the spectator, or that of *represented spatial* distance, i.e. the distance represented within the work. Less obvious, more metaphorical, is the

meaning of *temporal* distance. The first was noticed already by Aristotle in his *Poetics*; the second has played a great part in the history of painting in the form of perspective; the distinction between these two kinds of distance assumes special importance theoretically in the differentiation between sculpture in the round, and relief-sculpture. Temporal distance, remoteness from us in point of time, though often a cause of misconceptions, has been declared to be a factor of considerable weight in our appreciation.

It is not, however, in any of these meanings that 'Distance' is put forward here, though it will be clear in the course of this essay that the above mentioned kinds of distance are rather special forms of the conception of Distance as advocated here, and derive whatever *aesthetic* qualities they may possess from Distance in its *general* connotation. This general connotation is 'Psychical Distance.'

A short illustration will explain what is meant by 'Psychical Distance.' Imagine a fog at sea: for most people it is an experience of acute unpleasantness. Apart from the physical annoyance and remoter forms of discomfort such as delays, it is apt to produce feelings of peculiar anxiety, fears of invisible dangers, strains of watching and listening for distant and unlocalised signals. The listless movements of the ship and her warning calls soon tell upon the nerves of the passengers; and that special, expectant, tacit anxiety and nervousness, always associated with this experience, make a fog the dreaded terror of the sea (all the more terrifying because of its very silence and gentleness) for the expert seafarer no less than for the ignorant landsman.

Nevertheless, a fog at sea can be a source of intense relish and enjoyment. Abstract from the experience of the sea fog, for the moment, its danger and practical unpleasantness, just as every one in the enjoyment of a mountain-climb disregards its physical labour and its danger (though, it is not denied, that these may incidentally enter into the enjoyment and enhance it); direct the attention to the features 'objectively' constituting the phenomenon—the veil surrounding you with an opaqueness as of transparent milk, blurring the outline of things and distorting their shapes into weird grotesqueness; observe the carrying-power of the air, producing the impression as if you could touch some far-off siren by merely putting out your hand and letting it lose itself behind that white wall; note the curious creamy smoothness of the water, hypocritically denying as it were any suggestion of danger; and, above all, the strange solitude and remoteness from the world, as it can be found only on the highest mountain tops: and the experience may acquire, in its uncanny mingling of repose and terror, a flavour of such concentrated poignancy and delight as to contrast sharply with the blind and distempered anxiety of its other aspects. This contrast, often emerging with startling suddenness, is like a momentary switching on of some new current, or the passing ray of a brighter light, illuminating the outlook upon perhaps the most ordinary and familiar objects—an impression which we experience sometimes in instants of direst extremity, when our practical interest snaps like a wire from sheer over-tension, and we watch the consummation of some impending catastrophe with the marvelling unconcern of a mere spectator.

It is a difference of outlook, due—if such a metaphor is permissible—to the insertion of Distance. This Distance appears to lie between our own self and its affections, using the latter term in its broadest sense as anything which affects our being, bodily or spiritually, e.g. as sensation, perception, emotional state or idea. Usually, though not always, it amounts to the same thing to say that the Distance lies between our own self and such objects as are the sources or vehicles of such affections.

Thus, in the fog, the transformation by Distance is produced in the first instance by putting the phenomenon, so to speak, out of gear with our practical, actual self; by allowing it to stand outside the context of our personal needs and ends—in short, by looking at it 'objectively,' as it has often been called, by permitting only such reactions on our part as emphasise the 'objective' features of the experience, and by interpreting even our 'subjective' affections not as modes of *our* being but rather as characteristics of the phenomenon.

The working of Distance is, accordingly, not simple, but highly complex. It has a *negative*, inhibitory aspect—the cutting-out of the practical sides of things and of our practical attitude to them—and a *positive* side—the elaboration of the experience on the new basis created by the inhibitory action of Distance.

2. Consequently, this distanced view of things is not, and cannot be, our normal outlook. As a rule, experiences constantly turn the same side towards us, namely, that which has the strongest practical force of appeal. We are not ordinarily aware of those aspects of things which do not touch us immediately and practically, nor are we generally conscious of impressions apart from our own self which is impressed. The sudden view of things from their reverse, usually

unnoticed, side, comes upon us as a revelation, and such revelations are precisely those of Art. In this most general sense, Distance is a factor in all Art.

3. It is, for this very reason, also an aesthetic principle. The aesthetic contemplation and the aesthetic outlook have often been described as 'objective.' We speak of 'objective' artists as Shakespeare or Velasquez, of 'objective' works or art forms as Homer's *Iliad* or the drama. It is a term constantly occurring in discussions and criticisms, though its sense, if pressed at all, becomes very questionable. For certain forms of Art, such as lyrical poetry, are said to be 'subjective'; Shelley, for example, would usually be considered a 'subjective' writer. On the other hand, no work of Art can be genuinely 'objective' in the sense in which this term might be applied to a work on history or to a scientific treatise; nor can it be 'subjective' in the ordinary acceptance of that term, as a personal feeling, a direct statement of a wish or belief, or a cry of passion is subjective. 'Objectivity' and 'subjectivity' are a pair of opposites which in their mutual exclusiveness when applied to Art soon lead to confusion.

Nor are they the only pair of opposites. Art has with equal vigour been declared alternately 'idealistic' and 'realistic,' 'sensual' and 'spiritual,' 'individualistic' and 'typical.' Between the defence of either terms of such antitheses most aesthetic theories have vacillated. It is one of the contentions of this essay that such opposites find their synthesis in the more fundamental conception of Distance.

Distance further provides the much needed criterion of the beautiful as distinct from the merely agreeable.

Again, it marks one of the most important steps in the process of artistic creation and serves as a distinguishing feature of what is commonly so loosely described as the 'artistic temperament.'

Finally, it may claim to be considered as one of the essential characteristics of the 'aesthetic consciousness,'—if I may describe by this term that special mental attitude towards, and outlook upon, experience, which finds its most pregnant expression in the various forms of Art.

II.

Distance, as I said before, is obtained by separating the object and its appeal from one's own self, by putting it out of gear with practical needs and ends. Thereby the 'contemplation' of the object becomes alone possible. But it does not mean that the relation between the self and the object is broken to the extent of becoming 'impersonal.' Of the alternatives 'personal' and 'impersonal' the latter surely comes nearer to the truth; but here, as elsewhere, we meet the difficulty of having to express certain facts in terms coined for entirely different uses. To do so usually results in paradoxes, which are nowhere more inevitable than in discussions upon Art. 'Personal' and 'impersonal,' 'subjective' and 'objective' are such terms, devised for purposes other than aesthetic speculation, and becoming loose and ambiguous as soon as applied outside the sphere of their special meanings. In giving preference therefore to the term 'impersonal' to describe the relation between the spectator and a work of Art, it is to be noticed that it is not impersonal in the sense in which we speak of the 'impersonal' character of Science, for instance. In order to obtain 'objectively valid' results, the scientist excludes the 'personal factor,' i.e. his personal wishes as to the validity of his results, his predilection for any particular system to be proved or disproved by his research. It goes without saying that all experiments and investigations are undertaken out of a personal interest in the science, for the ultimate support of a definite assumption, and involve personal hopes of success; but this does not affect the 'dispassionate' attitude of the investigator, under pain of being accused of 'manufacturing his evidence.'

1. Distance does not imply an impersonal, purely intellectually interested relation of such a kind. On the contrary, it describes a *personal* relation, often highly emotionally coloured, but *of a peculiar character*. Its peculiarity lies in that the personal character of the relation has been, so to speak, filtered. It has been cleared of the practical, concrete nature of its appeal, without, however, thereby losing its original constitution. One of the best-known examples is to be found in our attitude towards the events and characters of the drama: they appeal to us like persons and incidents of normal experience, except that that side of their appeal, which would usually affect us in a directly personal manner, is held in abeyance. This difference, so well known as to be almost trivial, is generally

explained by reference to the knowledge that the characters and situations are 'unreal,' imaginary. In this sense Witasek[1], operating with Meinong's theory of *Annahmen*, has described the emotions involved in witnessing a drama as *Scheingefühle*, a term which has so frequently been misunderstood in discussions of his theories. But, as a matter of fact, the 'assumption' upon which the imaginative emotional reaction is based is not necessarily the condition, but often the consequence, of Distance; that is to say, the converse of the reason usually stated would then be true: viz. that Distance, by changing our relation to the characters, renders them seemingly fictitious, not that the fictitiousness of the characters alters our feelings toward them. It is, of course, to be granted that the actual and admitted unreality of the dramatic action reinforces the effect of Distance. But surely the proverbial unsophisticated yokel whose chivalrous interference in the play on behalf of the hapless heroine can only be prevented by impressing upon him that 'they are only pretending,' is not the ideal type of theatrical audience. The proof of the seeming paradox that it is Distance which primarily gives to dramatic action the appearance of unreality and not *vice versâ*, is the observation that the same filtration of our sentiments and the same seeming 'unreality' of *actual* men and things occur, when at times, by a sudden change of inward perspective, we are overcome by the feeling that "all the world's a stage."

2. This personal, but 'distanced' relation (as I will venture to call this nameless character of our view) directs attention to a strange fact which appears to be one of the fundamental paradoxes of Art: it is what I propose to call 'the antinomy of Distance.'

It will be readily admitted that a work of Art has the more chance of appealing to us the better it finds us prepared for its particular kind of appeal. Indeed, without some degree of predisposition on our part, it must necessarily remain incomprehensible, and to that extent unappreciated. The success and intensity of its appeal would seem, therefore, to stand in direct proportion to the completeness with which it corresponds with our intellectual and emotional peculiarities and the idiosyncracies of our experience. The absence of such a concordance between the characters of a work and of the spectator is, of course, the most general explanation for differences of 'tastes.'

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[1] H. Witasek, 'Zur psychologischen Analyse der aesthetischen Einfühlung,' *Ztsch. f. Psychol. u. Physiol. der Sinnesorg.* 1901, xxv. 1 ff.; *Grundzüge der Aesthetik*, Leipzig, 1904.

At the same time, such a principle of concordance requires a qualification, which leads at once to the antinomy of Distance.

Suppose a man, who believes that he has cause to be jealous about his wife, witnesses a performance of 'Othello.' He will the more perfectly appreciate the situation, conduct and character of Othello, the more exactly the feelings and experiences of Othello coincide with his own—at least he *ought* to on the above principle of concordance. In point of fact, he will probably do anything but appreciate the play. In reality, the concordance will merely render him acutely conscious of his own jealousy; by a sudden reversal of perspective he will no longer see Othello apparently betrayed by Desdemona, but himself in an analogous situation with his own wife. This reversal of perspective is the consequence of the loss of Distance. ✓

If this be taken as a typical case, it follows that the qualification required is that the coincidence should be as complete as is compatible with maintaining Distance. The jealous spectator of 'Othello' will indeed appreciate and enter into the play the more keenly, the greater the resemblance with his own experience—*provided* that he succeeds in keeping the Distance between the action of the play and his personal feelings: a very difficult performance in the circumstances. It is on account of the same difficulty that the expert and the professional critic make a bad audience, since their expertness and critical professionalism are *practical* activities, involving their concrete personality and constantly endangering their Distance. [It is, by the way, one of the reasons why Criticism is an art, for it requires the constant interchange from the practical to the distanced attitude and *vice versâ*, which is characteristic of artists.]

The same qualification applies to the artist. He will prove artistically most effective in the formulation of an intensely *personal* experience, but he can formulate it artistically only on condition of a detachment from the experience *quâ personal*. Hence the statement of so many artists that artistic formulation was to them a kind of catharsis, a means of ridding themselves of feelings and ideas the acuteness of which they felt almost as a kind of obsession. Hence, on the other hand, the failure of the average man to convey to others at all adequately the impression of an overwhelming joy or sorrow. His personal implication in the event renders it impossible for him to formulate and present it in such a way as to make others, like himself, feel all the meaning and fulness which it possesses for him.

What is therefore, both in appreciation and production, most desirable is the *utmost decrease of Distance without its disappearance.*

3. Closely related, in fact a presupposition to the 'antimony,' is the *variability of Distance.* Herein especially lies the advantage of Distance compared with such terms as 'objectivity' and 'detachment.' Neither of them implies a *personal* relation—indeed both actually preclude it; and the mere inflexibility and exclusiveness of their opposites render their application generally meaningless.

Distance, on the contrary, admits naturally of degrees, and differs not only according to the nature of the *object,* which may impose a greater or smaller degree of Distance, but varies also according to the *individual's capacity* for maintaining a greater or lesser degree. And here one may remark that not only do *persons differ from each other* in their habitual measure of Distance, but that the *same individual differs* in his ability to maintain it in the face of different objects and of different arts.

There exist, therefore, two different sets of conditions affecting the degree of Distance in any given case: those offered by the object and those realised by the subject. In their interplay they afford one of the most extensive explanations for varieties of aesthetic experience, since loss of Distance, whether due to the one or the other, means loss of aesthetic appreciation.

In short, Distance may be said *to be variable both according to the distancing-power of the individual, and according to the character of the object.*

There are two ways of losing Distance: either to 'under-distance' or to 'over-distance.' 'Under-distancing' is the commonest failing of the *subject,* an excess of Distance is a frequent failing of *Art,* especially in the past. Historically it looks almost as if Art had attempted to meet the deficiency of Distance on the part of the subject and had overshot the mark in this endeavour. It will be seen later that this is actually true, for it appears that over-distanced Art is specially designed for a class of appreciation which has difficulty to rise spontaneously to any degree of Distance. The consequence of a loss of Distance through one or other cause is familiar: the verdict in the case of under-distancing is that the work is 'crudely naturalistic,' 'harrowing,' 'repulsive in its realism.' An excess of Distance produces the impression of improbability, artificiality, emptiness or absurdity.

The individual tends, as I just stated, to under-distance rather than to lose Distance by over-distancing. *Theoretically* there is no limit to the decrease of Distance. In theory, therefore, not only the usual subjects of Art, but even the most personal affections, whether ideas, percepts or emotions, can be sufficiently distanced to be aesthetically appreciable. Especially artists are gifted in this direction to a remarkable extent. The average individual, on the contrary, very rapidly reaches his limit of decreasing Distance, his 'Distance-limit,' i.e. that point at which Distance is lost and appreciation either disappears or changes its character.

In the *practice*, therefore, of the average person, a limit does exist which marks the minimum at which his appreciation can maintain itself in the aesthetic field, and this average minimum lies considerably higher than the Distance-limit of the artist. It is practically impossible to fix this average limit, in the absence of data, and on account of the wide fluctuations from person to person to which this limit is subject. But it is safe to infer that, in art practice, explicit references to organic affections, to the material existence of the body, especially to sexual matters, lie normally below the Distance-limit, and can be touched upon by Art only with special precautions. Allusions to social institutions of any degree of personal importance—in particular, allusions implying any doubt as to their validity—the questioning of some generally recognised ethical sanctions, references to topical subjects occupying public attention at the moment, and such like, are all dangerously near the average limit and may at any time fall below it, arousing, instead of aesthetic appreciation, concrete hostility or mere amusement.

This difference in the Distance-limit between artists and the public has been the source of much misunderstanding and injustice. Many an artist has seen his work condemned, and himself ostracized for the sake of so-called 'immoralities' which to him were *bonâ fide* aesthetic objects. His power of distancing, nay, the necessity of distancing feelings, sensations, situations which for the average person are too intimately bound up with his concrete existence to be regarded in that light, have often quite unjustly earned for him accusations of cynicism, sensualism, morbidness or frivolity. The same misconception has arisen over many 'problem plays' and 'problem novels' in which the public have persisted in seeing nothing but a supposed 'problem' of the moment, whereas the author may have been—and often has demonstrably been—able to distance the subject-matter sufficiently to rise above its practical problematic import and to regard it simply as a dramatically and humanly interesting situation.

The variability of Distance in respect to Art, disregarding for the moment the subjective complication, appears both as a general feature in Art, and in the differences between the special arts.

It has been an old problem why the 'arts of the eye and of the ear' should have reached the practically exclusive predominance over arts of other senses. Attempts to raise 'culinary art' to the level of a Fine Art have failed in spite of all propaganda, as completely as the creation of scent or liqueur 'symphonies.' There is little doubt that, apart from other excellent reasons[1] of a partly psycho-physical, partly technical nature, the actual, *spatial distance* separating objects of sight and hearing from the subject has contributed strongly to the development of this monopoly. In a similar manner *temporal remoteness* produces Distance, and objects removed from us in point of time are *ipso facto* distanced to an extent which was impossible for their contemporaries. Many pictures, plays and poems had, as a matter of fact, rather an expositary or illustrative significance—as for instance much ecclesiastical Art—or the force of a direct practical appeal—as the invectives of many satires or comedies—which seem to us nowadays irreconcilable with their aesthetic claims. Such works have consequently profited greatly by lapse of time and have reached the level of Art only with the help of temporal distance, while others, on the contrary, often for the same reason have suffered a loss of Distance, through *over*-distancing.

Special mention must be made of a group of artistic conceptions which present excessive Distance in their form of appeal rather than in their actual presentation—a point illustrating the necessity of distinguishing between distancing an object and distancing the appeal of which it is the source. I mean here what is often rather loosely termed 'idealistic Art,' that is, Art springing from abstract conceptions, expressing allegorical meanings, or illustrating general truths. Generalisations and abstractions suffer under this disadvantage that they have too much general applicability to invite a personal interest in them, and too little individual concreteness to prevent them applying to us in all their force. They appeal to everybody and therefore to none. An axiom of Euclid belongs to nobody, just because it compels everyone's assent; general conceptions like Patriotism, Friendship, Love, Hope, Life, Death, concern as much Dick, Tom and Harry as myself, and I, therefore, either feel unable to get into any kind of personal relation to them, or, if I do so, they become at once, emphatically and concretely, *my* Patriotism, *my* Friendship, *my*

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[1] J. Volkelt, 'Die Bedeutung der niederen Empfindungen für die aesthetische Einfühlung,' *Ztsch. für Psychol. u. Physiol. der Sinnesorg.* xxxii. 15, 16; *System der Aesthetik*, 1905, i. 260 ff.

Love, *my* Hope, *my* Life and Death. By mere force of generalisation, a general truth or a universal ideal is so far distanced from myself that I fail to realise it concretely at all, or, when I do so, I can realise it only as part of my *practical actual being*, i.e. it falls below the Distance-limit altogether. 'Idealistic Art' suffers consequently under the peculiar difficulty that its excess of Distance turns generally into an *under*-distanced appeal—all the more easily, as it is the usual failing of the subject to *under-* rather than to *over*-distance.

The different special arts show at the present time very marked variations in the degree of Distance which they usually impose or require for their appreciation. Unfortunately here again the absence of data makes itself felt and indicates the necessity of conducting observations, possibly experiments, so as to place these suggestions upon a securer basis. In one single art, viz. the *theatre*, a small amount of information is available, from an unexpected source, namely the proceedings of the censorship committee[1], which on closer examination might be made to yield evidence of interest to the psychologist. In fact, the whole censorship problem, as far as it does not turn upon purely economic questions, may be said to hinge upon Distance; if every member of the public could be trusted to keep it, there would be no sense whatever in the existence of a censor of plays. There is, of course, no doubt that, speaking generally, theatrical performances *eo ipso* run a special risk of a loss of Distance owing to the material presentment[2] of its subject-matter. The physical presence of living human beings as vehicles of dramatic art is a difficulty which no art has to face in the same way. A similar, in many ways even greater, risk confronts *dancing*: though attracting perhaps a less widely spread human interest, its animal spirits are frequently quite unrelieved by any glimmer of spirituality and consequently form a proportionately stronger lure to under-distancing. In the higher forms of dancing technical execution of the most wearing kind makes up a great deal for its intrinsic tendency towards a loss of Distance, and as a popular performance, at least in southern Europe, it has retained much of its ancient artistic glamour, producing a peculiarly subtle balancing of Distance between the pure delight of bodily movement and high technical accomplishment. In passing, it is interesting to observe (as bearing upon the development of Distance), that this art, once as much a fine art as music and considered by the Greeks as a particularly valuable educational exercise, should—except in sporadic cases—have fallen so low from the pedestal it once occupied. Next to the theatre and dancing stands *sculpture.* Though not using a *living* bodily medium, yet the human form in its full spatial materiality constitutes a similar threat to Distance. Our northern habits of dress and ignorance of the human body have enormously increased the difficulty of distancing Sculpture, in part through the gross misconceptions to which it is exposed, in part owing to a complete lack of standards of bodily perfection, and an inability to realise the distinction between sculptural form and bodily shape, which is the only but fundamental point distinguishing a statue from a cast taken from life. In *painting* it is apparently the form of its presentment and the usual reduction in scale which would explain why this art can venture to approach more closely than sculpture to the normal Distance-limit. As this matter will be discussed later in a special connexion this simple reference may suffice here. *Music* and *architecture* have a curious position. These two most abstract of all arts show a remarkable fluctuation in their Distances. Certain kinds of music, especially 'pure' music, or 'classical' or 'heavy' music, appear for many people over-distanced; light, 'catchy' tunes, on the contrary, easily reach that degree of decreasing Distance below which they cease to be Art and become a pure amusement. In spite of its strange abstractness which to many philosophers has made it comparable to architecture and mathematics, music possesses a sensuous, frequently sensual, character: the undoubted physiological and muscular stimulus of its melodies and harmonies, no less than its rhythmic aspects, would seem to account for the occasional disappearance of Distance. To this might be added its strong tendency, especially in unmusical people, to stimulate trains of thought quite disconnected with itself, following channels of subjective inclinations,—day-dreams of a more or less directly personal character. *Architecture* requires almost uniformly a very great Distance; that is to say, the majority of persons derive no aesthetic appreciation from architecture as such, apart from the incidental impression of its decorative features and its associations. The causes are numerous, but prominent among them are the confusion of building with architecture and the predominance of utilitarian purposes, which overshadow the architectural claims upon the attention.

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[1] Report from the Joint Select Committee of the House of Lords and the House of Commons on the Stage Plays (Censorship), 1909.

[2] I shall use the term 'presentment' to denote the manner of presenting, in distinction to 'presentation' as that which is presented.

4. That all art requires a Distance-limit beyond which, and a Distance within which only, aesthetic appreciation becomes possible, is the *psychological formulation of a general characteristic of Art,* viz. its

anti-realistic nature. Though seemingly paradoxical, this applies as much to 'naturalistic' as to 'idealistic' Art. The difference commonly expressed by these epithets is at bottom merely the difference in the degree of Distance; and this produces, so far as 'naturalism' and 'idealism' in Art are not meaningless labels, the usual result that what appears obnoxiously 'naturalistic' to one person, may be 'idealistic' to another. To say that Art is anti-realistic simply insists upon the fact that Art is not nature, never pretends to be nature and strongly resists any confusion with nature. It emphasizes the *art*-character of Art: 'artistic' is synonymous with 'anti-realistic'; it explains even sometimes a very marked degree of artificiality.

"Art is an imitation of nature," was the current art-conception in the 18th century. It is the fundamental axiom of the standard-work of that time upon aesthetic theory by the Abbé Du Bos, *Réflexions critiques sur la poésie et la peinture,* 1719; the idea received strong support from the literal acceptance of Aristotle's theory of μίμησις and produced echoes everywhere, in Lessing's *Laocoon* no less than in Burke's famous statement that "all Art is great as it deceives." Though it may be assumed that since the time of Kant and of the Romanticists this notion has died out, it still lives in unsophisticated minds. Even when formally denied, it persists, for instance, in the belief that "Art idealises nature," which means after all only that Art copies nature with certain improvements and revisions. Artists themselves are unfortunately often responsible for the spreading of this conception. Whistler indeed said that to produce Art by imitating nature would be like trying to produce music by sitting upon the piano, but the selective, idealising imitation of nature finds merely another support in such a saying. Naturalism, pleinairism, impressionism,—even the guileless enthusiasm of the artist for the works of nature, her wealth of suggestion, her delicacy of workmanship, for the steadfastness of her guidance, only produce upon the public the impression that Art is, after all, an imitation of nature. Then how can it be anti-realistic? The antithesis, Art *versus* nature, seems to break down. Yet if it does, what is the sense of Art?

Here the conception of Distance comes to the rescue. The solution of the dilemma lies in the 'antinomy of Distance' with its demand: utmost decrease of Distance without its disappearance. The simple observation that Art is the more effective, the more it falls into line with our predispositions which are inevitably moulded on general experience and nature, has always been the original motive for 'naturalism.' 'Naturalism,' 'impressionism' is no new thing; it is only a new name for an innate leaning of Art, from the time of the Chaldeans and Egyptians down to the present day. Even the Apollo of Tenea apparently struck his contemporaries as so startlingly 'naturalistic' that the subsequent legend attributed a superhuman genius to his creator. A constantly closer approach to nature, a perpetual refining of the limit of Distance, yet without overstepping the dividing line of art and nature, has always been the inborn bent of art. To deny this dividing line has occasionally been the failing of naturalism. But no theory of naturalism is complete which does not at the same time allow for the intrinsic idealism of Art: for both are merely degrees in that wide range lying beyond the Distance-limit. To imitate nature so as to trick the spectator into the deception that it is nature which he beholds, is to forsake Art, its anti-realism, its distanced spirituality, and to fall below the limit into sham, sensationalism or platitude.

But what, in the theory of antinomy of Distance requires explanation is the existence of an *idealistic, highly distanced* Art. There are numerous reasons to account for it; indeed in so complex a phenomenon as Art, *single* causes can be pronounced almost *a priori* to be false. Foremost among such causes which have contributed to the formation of an idealistic Art appears to stand the subordination of Art to some extraneous purpose of an impressive, exceptional character. Such a subordination has consisted—at various epochs of Art history—in the use to which Art was put to subserve commemorative, hieratic, generally religious, royal or patriotic functions. The object to be commemorated had to stand out from among other still existing objects or persons; the thing or the being to be worshipped had to be distinguished as markedly as possible from profaner objects of reverence and had to be invested with an air of sanctity by a removal from its ordinary context of occurrence. Nothing could have assisted more powerfully the introduction of a high Distance than this attempt to differentiate objects of common experience in order to fit them for their exalted position. Curious, unusual things of nature met this tendency half-way and easily assumed divine rank; but others had to be distanced by an exaggeration of their size, by extraordinary attributes, by strange combinations of human and animal forms, by special insistence upon particular characteristics, or by the careful removal of all noticeably individualistic and concrete features. Nothing could be more striking than the contrast, for example, in Egyptian Art between the monumental, stereotyped effigies of the Pharaohs, and the startlingly realistic rendering of domestic scenes and of ordinary mortals, such as "the Scribe" or "the Village Sheikh." Equally noteworthy is the exceeding artificiality of Russian eikon-painting with its prescribed attributes, expressions and gestures. Even Greek dramatic practice appears to have aimed, for similar purposes and in marked contrast to our stage-habits, at an increase rather than at a decrease of Distance. Otherwise Greek Art, even of a religious type, is remarkable for its *low* Distance value; and it speaks highly for the aesthetic capacities of the Greeks that the degree of realism which they ventured to impart to the representations of their gods, while humanising them, did not, at least at first[1], impair the reverence of their feelings towards them. But apart from such special causes, idealistic Art of great Distance has appeared at intervals, for apparently no other reason than that the great Distance was felt to be essential to its *art*-character. What is noteworthy and runs counter to many accepted ideas is that such periods were usually epochs of a low level of general culture. These were times, which, like childhood, required the marvellous, the extraordinary, to satisfy their artistic longings, and neither realised nor cared for the poetic or artistic qualities of ordinary things. They were frequently times, in which the mass of the people were plunged in ignorance and buried under a load of misery, and in which even the small educated class sought rather amusement or a pastime in Art; or they were epochs of a strong practical commonsense too much concerned with the rough-and-tumble of life to have any sense of its aesthetic charms. Art was to them what melodrama is to a section of the public at the present time, and its wide Distance was the safeguard of its artistic character. The flowering periods of Art have, on the contrary, always borne the evidence of a narrow Distance. Greek Art, as just mentioned, was realistic to an extent which we, spoilt as we are by modern developments, can grasp with difficulty, but which the contrast with its oriental contemporaries sufficiently proves. During the Augustan period—which Art historians at last are coming to regard no longer as merely 'degenerated' Greek Art—Roman Art achieved its greatest triumphs in an almost naturalistic portrait-sculpture. In the Renaissance we need only think of the realism of portraiture, sometimes amounting almost to cynicism, of the *désinvolture* with which the mistresses of popes and dukes were posed as madonnas, saints and goddesses apparently without any detriment to the aesthetic appeal of the works, and of the remarkable interpenetration of Art with the most ordinary routine of life, in order to realise the scarcely perceptible dividing line between the sphere of Art and the realm of practical existence. In a sense, the assertion that idealistic Art marks periods of a generally low and narrowly restricted culture is the converse to the oft-repeated statement that the flowering periods of Art coincide with epochs of decadence: for this so-called decadence represents indeed in certain respects a process of disintegration, politically, racially, often nationally, but a disruption necessary to the formation of larger social units and to the breakdown of out-grown national restrictions. For this very reason it has usually also been the sign of the growth of personal independence and of an expansion of individual culture.

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[1] That this practice did, in course of time, undermine their religious faith, is clear from the plays of Euripides and from Plato's condemnation of Homer's mythology.

To proceed to some more special points illustrating the distanced and therefore anti-realistic character of art,—both in subject-matter and in the form of presentation Art has always safeguarded its distanced view. Fanciful, even phantastic, subjects have from time immemorial been the accredited material of Art. No doubt things, as well as our view of them, have changed in the course of time: *Polyphemus* and the *Lotus-Eaters* for the Greeks, the *Venusberg* or the *Magnetic Mountain* for the Middle Ages were less incredible, more realistic than to us. But *Peter Pan* or *L'Oiseau Bleu* still appeal at the present day in spite of the prevailing note of realism of our time. 'Probability' and 'improbability' in Art are not to be measured by their correspondence (or lack of it) with actual experience. To do so had involved the theories of the 15th to the 18th centuries in endless contradictions. It is rather a matter of *consistency* of Distance. The note of realism, set by a work as a whole, determines *intrinsically* the greater or smaller degree of fancy which it permits; and consequently we feel the loss of Peter Pan's shadow to be infinitely more probable than some trifling improbability which shocks our sense of proportion in a naturalistic work. No doubt also, fairy-tales, fairy-plays, stories of strange adventures were primarily invented to satisfy the craving of curiosity, the desire for the marvellous, the shudder of the unwonted and the longing for imaginary experiences. But by their mere eccentricity in regard to the normal facts of experience they cannot have failed to arouse a strong feeling of Distance.

Again, certain conventional subjects taken from mythical and legendary traditions, at first closely connected with the concrete, practical, life of a devout public, have gradually, by the mere force of convention as much as by their inherent anti-realism, acquired Distance for us to-day. Our view of Greek mythological sculpture, of early Christian saints and martyrs must be considerably distanced, compared with that of the Greek and medieval worshipper. It is in part the result of lapse of time, but in part also a real change of attitude. Already the outlook of the Imperial Roman had altered, and Pausanias shows a curious dualism of standpoint, declaring the Athene Lemnia to be the supreme achievement of Phidias's genius, and gazing awe-struck upon the roughly hewn tree-trunk representing some primitive Apollo. Our understanding of Greek tragedy suffers admittedly under our inability to revert to the point of view for which it was originally written. Even the tragedies of Racine demand an imaginative effort to put ourselves back into the courtly atmosphere of red-heeled, powdered ceremony. Provided the Distance is not too wide, the result of its intervention has everywhere been to enhance the *art*-character of such works and to lower their original ethical and social force of appeal. Thus in the central dome of the Church (Sta Maria dei Miracoli) at Saronno are depicted the heavenly hosts in ascending tiers, crowned by the benevolent figure of the Divine Father, bending from the window of heaven to bestow His blessing upon the assembled community. The mere realism of foreshortening and of the boldest vertical perspective may well have made the naïve Christian of the 16th century conscious of the Divine Presence—but for us it has become a work of Art.

The unusual, exceptional, has found its especial home in tragedy. It has always—except in highly distanced tragedy—been a popular objection to it that 'there is enough sadness in life without going to the theatre for it.' Already Aristotle appears to have met with this view among his contemporaries clamouring for 'happy endings.' Yet tragedy is not sad; if it were, there would indeed be little sense in its existence. For the tragic is just in so far different from the merely sad, as it is distanced; and it is largely the exceptional which produces the Distance of tragedy: exceptional situations, exceptional characters, exceptional destinies and conduct. Not of course, characters merely cranky, eccentric, pathological. The exceptional element in tragic figures—that which makes them so utterly different from characters we meet with in ordinary experience—is a consistency of direction, a fervour of ideality, a persistence and driving-force which is far above the capacities of average men. The tragic of tragedy would, transposed into ordinary life, in nine cases out of ten, end in drama, in comedy, even in farce, for lack of steadfastness, for fear of conventions, for the dread of 'scenes,' for a hundred-and-one petty faithlessnesses towards a belief or an ideal:

even if for none of these, it would end in a compromise simply because man forgets and time heals[1]. Again, the sympathy, which aches with the sadness of tragedy is another such confusion, the under-distancing of tragedy's appeal. Tragedy trembles always on the knife-edge of a *personal* reaction, and sympathy which finds relief in tears tends almost always towards a loss of Distance. Such a loss naturally renders tragedy unpleasant to a degree: it becomes sad, dismal, harrowing, depressing. But real tragedy (melodrama has a very strong tendency to speculate upon sympathy), truly appreciated, is not sad. "The pity of it—oh, the pity of it," that essence of all genuine tragedy is not the pity of mild, regretful sympathy. It is a chaos of tearless, bitter bewilderment, of upsurging revolt and rapturous awe before the ruthless and inscrutable fate; it is the homage to the great and exceptional in the man who in a last effort of spiritual tension can rise to confront blind, crowning Necessity even in his crushing defeat.

As I explained earlier, the form of presentation sometimes endangers the maintenance of Distance, but it more frequently acts as a considerable support. Thus the bodily vehicle of *drama* is the chief factor of risk to Distance. But, as if to counterbalance a confusion with nature, other features of stage-presentation exercise an opposite influence. Such are the general theatrical *milieu*, the shape and arrangement of the stage, the artificial lighting, the costumes, *mise-en-scène* and make-up, even the language, especially verse. Modern reforms of staging, aiming primarily at the removal of artistic incongruities between excessive decoration and the living figures of the actors and at the production of a more homogeneous stage-picture, inevitably work also towards a greater emphasis and homogeneity of Distance. The history of staging and dramaturgy is closely bound up with the evolution of Distance, and its fluctuations lie at the bottom not only of the greater part of all the talk and writing about 'dramatic probability' and the Aristotelian 'unities,' but also of 'theatrical illusion.' In *sculpture*, one distancing factor of presentment is its lack of colour. The aesthetic, or rather inaesthetic effect of realistic colouring, is in no way touched by the controversial question of its use historically; its attempted resuscitation, such as by Klinger, seems only to confirm its disadvantages. The distancing use even of pedestals, although originally no doubt serving other purposes, is evident to anyone who has experienced the oppressively crowded sensation of moving in a room among life-sized statues placed directly upon the floor. The circumstance that the space of statuary is the same space as ours (in distinction to relief sculpture or painting, for instance) renders a distancing by pedestals, i.e. a removal from our spatial context, imperative[1]. Probably the framing of *pictures* might be shown to serve a similar purpose—though paintings have intrinsically a much greater Distance—because neither their space (perspective and imaginary space) nor their lighting coincides with our (actual) space or light, and the usual reduction in scale of the represented objects prevents a feeling of undue proximity. Besides, painting always retains to some extent a *two*-dimensional character, and this character supplies *eo ipso* a Distance. Nevertheless, life-size pictures, especially if they possess strong relief, and their light happens to coincide with the actual lighting, can occasionally produce the impression of actual presence which is a far from pleasant, though fortunately only a passing, illusion. For decorative purposes, in pictorial renderings of vistas, garden-perspectives and architectural extensions, the removal of Distance has often been consciously striven after, whether with aesthetically satisfactory results is much disputed.

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[1] The famous 'unity of time,' so senseless as a 'canon,' is all the same often an indispensable condition of tragedy. For in many a tragedy the catastrophe would be even intrinsically impossible, if fatality did not overtake the hero with that rush which gives no time to forget and none to heal. It is in cases such as these that criticism has often blamed the work for 'improbability'—the old confusion between Art and nature—forgetting that the death of the hero is the convention of the art-form, as much as grouping in a picture is such a convention and that probability is not the correspondence with average experience, but consistency of Distance.

A general help towards Distance (and therewith an anti-realistic feature) is to be found in the 'unification of presentment[2]' of all art-objects. By unification of presentment are meant such qualities as symmetry, opposition, proportion, balance, rhythmical distribution of parts, light-arrangements, in fact all so-called 'formal' features, 'composition' in the widest sense. Unquestionably, Distance is not the only, nor even the principal function of composition; it serves to render our grasp of the presentation easier and to increase its intelligibility. It may even in itself constitute the principal aesthetic feature of the object, as in linear complexes or patterns, partly also in architectural designs. Yet, its distancing effect can hardly be underrated. For, every kind of visibly intentional arrangement or unification must, by the mere fact of its presence, enforce Distance, by distinguishing the object from the confused, disjointed and scattered forms of actual experience. This function can be gauged in a typical form in cases where composition produces an exceptionally marked impression of artificiality (not in the bad sense of that term, but in the sense in which all art is artificial); and it is a natural corollary to the differences of Distance in different arts and of different subjects, that the arts and subjects vary in the degree of artificiality which they can bear. It is this sense of artificial finish which is the source of so much of that elaborate charm of Byzantine work, of Mohammedan decoration, of the hieratic stiffness of so many primitive madonnas and saints. In general the emphasis of composition and technical finish increases with the Distance of the subject-matter: heroic conceptions lend themselves better to verse than to prose; monumental statues require a more general treatment, more elaboration of setting and artificiality of pose than impressionistic statuettes like those of Troubetzkoi; an ecclesiastic subject is painted with a degree of symmetrical arrangement which would be ridiculous in a Dutch interior, and a naturalistic drama carefully avoids the tableau impression characteristic of a mystery play. In a similar manner the variations of Distance in the arts go hand in hand with a visibly greater predominance of composition and 'formal' elements, reaching a climax in architecture and music. It is again a matter of 'consistency of Distance.' At the same time, while from the point of view of the artist this is undoubtedly the case, from the point of view of the public the emphasis of composition and technical finish appears frequently to relieve the impression of highly distanced subjects by *diminishing the Distance of the whole.* The spectator has a tendency to see in composition and finish merely evidence of the artist's 'cleverness,' of his mastery over his material. Manual dexterity is an enviable thing to possess in everyone's experience, and naturally appeals to the public *practically*, thereby putting it into a directly personal relation to things which intrinsically have very little personal appeal for it. It is true that this function of composition is hardly an aesthetic one: for the admiration of mere technical cleverness is not an artistic enjoyment, but by a fortunate chance it has saved from oblivion and entire loss, among much rubbish, also much genuine Art, which otherwise would have completely lost contact with our life.

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[1] An instance which might be adduced to disprove this point only shows its correctness on closer inspection: for it was on purpose and with the intention of removing Distance, that Rodin originally intended his *citoyens de Calais* to be placed, without pedestals, upon the market-place of that town.

[2] See note 2, p. 97.

5. This discussion, necessarily sketchy and incomplete, may have helped to illustrate the sense in which, I suggested, Distance appears

as a fundamental principle to which such antitheses as idealism and realism are reducible. The difference between 'idealistic' and 'realistic' Art is not a clear-cut dividing-line between the art-practices described by these terms, but is a difference of degree in the Distance-limit which they presuppose on the part both of the artist and of the public. A similar reconciliation seems to me possible between the opposites 'sensual' and 'spiritual,' 'individual' and 'typical.' That the appeal of Art is sensuous, even sensual, must be taken as an indisputable fact. Puritanism will never be persuaded, and rightly so, that this is not the case. The sensuousness of Art is a natural implication of the 'antinomy of Distance,' and will appear again in another connexion. The point of importance here is that the whole sensual side of Art is purified, spiritualised, 'filtered' as I expressed it earlier, by Distance. The most sensual appeal becomes the translucent veil of an underlying spirituality, once the grossly personal and practical elements have been removed from it. And—a matter of special emphasis here—*this spiritual aspect of the appeal is the more penetrating, the more personal and direct its sensual appeal would have been* BUT FOR THE PRESENCE OF DISTANCE. For the artist, to trust in this delicate transmutation is a natural act of faith which the Puritan hesitates to venture upon: which of the two, one asks, is the greater idealist?

6. The same argument applies to the contradictory epithets 'individual' and 'typical.' A discussion in support of the fundamental individualism of Art lies outside the scope of this essay. Every artist has taken it for granted. Besides it is rather in the sense of 'concrete' or 'individualised,' that it is usually opposed to 'typical.' On the other hand, 'typical,' in the sense of 'abstract,' is as diametrically opposed to the whole nature of Art, as individualism is characteristic of it. It is in the sense of 'generalised' as a 'general human element' that it is claimed as a necessary ingredient in Art. This antithesis is again one which naturally and without mutual sacrifice finds room within the conception of Distance. Historically the 'typical' has had the effect of counteracting *under*-distancing as much as the 'individual' has opposed *over*-distancing. Naturally the two ingredients have constantly varied in the history of Art; they represent, in fact, two sets of conditions to which Art has invariably been subject: the personal and the social factors. It is Distance which on one side prevents the emptying of Art of its concreteness and the development of the typical into abstractness; which, on the other, suppresses the directly personal element of its individualism; thus reducing the antitheses to the peaceful interplay

of these two factors. It is just this interplay which constitutes the 'antinomy of Distance.'

III.

It remains to indicate the value of Distance as *an aesthetic principle*: as criterion in some of the standing problems of Aesthetics; as representing a phase of artistic creation; and as a characteristic feature of the 'aesthetic consciousness.'

1. The axiom of 'hedonistic Aesthetics' is that beauty is pleasure. Unfortunately for hedonism the formula is not reversible: not all pleasure is beauty. Hence the necessity of some limiting criterion to separate the beautiful within the 'pleasure-field' from the merely agreeable. This relation of the beautiful to the agreeable is the ever recurring crux of all hedonistic Aesthetics, as the problem of this relation becomes inevitable when once the hedonistic basis is granted. It has provoked a number of widely different solutions, some manifestly wrong, and all as little satisfactory as the whole hedonistic groundwork upon which they rest: the shareableness of beauty as opposed to the 'monopoly' of the agreeable (Bain)[1], the passivity of beauty-pleasure (Grant Allen)[2], or most recently, the 'relative permanence of beauty-pleasure in revival' (H. R. Marshall)[3].

Distance offers a distinction which is as simple in its operation as it is fundamental in its importance: *the agreeable is a non-distanced pleasure.* Beauty in the widest sense of aesthetic value is impossible without the insertion of Distance. The agreeable stands in precisely the same relation to the beautiful (in its narrower sense) as the sad stands to the tragic, as indicated earlier. Translating the above formula, one may say, that the agreeable is felt as an affection of our concrete, practical self; the centre of gravity of an agreeable experience lies in the self which experiences the agreeable. The aesthetic experience, on the contrary, has its centre of gravity in itself or in the object mediating it, not in the self which has been distanced out of the field of the inner vision of the experiencer: "not the fruit of experience, but experience itself, is the end." It is for this reason that to be asked in the midst of an intense aesthetic impression "whether one likes it," is like a somnambulist being called by name: it is a recall to one's concrete self, an awakening of practical consciousness which throws the whole aesthetic mechanism out of gear. One might almost venture upon the paradox that the more intense the aesthetic absorption, the less one "likes," consciously, the experience. The failure to realise this fact, so fully borne out by all genuine artistic experience, is the fundamental error of hedonistic Aesthetics.

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[1] Bain, *The Emotions and the Will*, 2nd ed. 1850.

[2] G. Allen, *Physiological Aesthetics*, 1897.

[3] H. R. Marshall, *Pain, Pleasure and Aesthetics*, 1894; *Aesthetic Principles*, 1895.

The problem of the relation of the beautiful and the agreeable has taken more definite shape in the question of the aesthetic value of the so-called 'lower senses' (comprising sensations of taste and temperature, muscular and tactile, and organic sensations). Sight and hearing have always been the 'aesthetic senses' *par excellence*. Scent has been admitted to the status of an aesthetic sense by some, excluded by others. The ground for the rejection of the lower senses has always been that they mediate only agreeable sensations, but are incapable of conveying aesthetic experiences. Though true normally, this rigid distinction is theoretically unfair to the senses, and in practice often false. It is undoubtedly very difficult to reach an aesthetic appreciation through the lower senses, because the materialness of their action, their proximity and bodily connexion are great obstacles to their distancing. The aroma of coffee may be a kind of foretaste, taste etherialised, but still a taste. The sweetness of scent of a rose is usually felt more as a bodily caress than as an aesthetic experience. Yet poets have not hesitated to call the scents of flowers their "souls." Shelley has transformed the scent to an imperceptible sound[1]. We call such conceptions 'poetical': they mark the transition from the merely agreeable to the beautiful by means of Distance.

M. Guyau, in a well-known passage[2], has described the same transformation of a taste. Even muscular sensations may present aesthetic possibilities, in the free exercise of bodily movement, the swing of a runner, in the ease and certainty of the trained gymnast; nay, such diffuse organic sensations as the buoyancy of well-being, and the elasticity of bodily energy, can, in privileged moments, be aesthetically enjoyed. That they admit of no material fixation, such as objects of sight and hearing do, and for that reason form no part of Art in the narrower sense; that they exist as aesthetic objects only for the moment and for the single being that enjoys them, is no argument against their aesthetic character. Mere material existence and permanence is no aesthetic criterion.

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[1] Cf. "The Sensitive Plant."

[2] M. Guyau, *Problèmes de l'Esthétique contemporaine*, Paris, 1897, 4^{me} ed. Livre I. chap. VI.

This is all the more true, as even among the experiences of lasting things, such as are generally accounted to yield aesthetic impressions, the merely agreeable occurs as frequently as the beautiful.

To begin with the relatively simple case of colour-appreciation. Most people imagine that because they are not colour-blind, physically or spiritually, and prefer to live in a coloured world rather than in an engraving, they possess an aesthetic appreciation of colour as such. This is the sort of fallacy which hedonistic art-theories produce, and the lack of an exchange of views on the subject only fosters. Everybody believes that he enjoys colour—and for that matter other things—just like anyone else. Yet rather the contrary is the case. By far the greater number, when asked why they like a colour, will answer, that they like it, because it strikes them as warm or cold, stimulating or soothing, heavy or light. They constitute a definite type of colour-appreciation and form about sixty per cent. of all persons. The remainder assumes, for the greater part, a different attitude. Colours do not appeal to them as effects (largely organic) upon themselves. Their appreciation attributes to colours a kind of personality: colours are energetic, lively, serious, pensive, melancholic, affectionate, subtle, reserved, stealthy, treacherous, brutal, etc. These characters are not mere imaginings, left to the whim of the individual, romancing whatever he pleases into the colours, nor are they the work simply of accidental associations. They follow, on the contrary, definite rules in their applications; they are, in fact, the same organic effects as those of the former type, but transformed into, or interpreted as, attributes of the colour, instead of as affections of one's own self. In short, they are the result of the distancing of the organic effects: they form an aesthetic appreciation of colour, instead of a merely agreeable experience like those of the former kind[1].

A similar parallelism of the agreeable and the beautiful (in the widest sense of aesthetic value) occurs also within the sphere of recognised art-forms. I select for special notice *comedy* and *melodrama* (though the same observation can be made in painting, architecture and notably in music), firstly as counterparts to tragedy, discussed earlier, secondly, because both represent admitted art-forms, in spite of their at least partially, inadequate claims to the distinction, and lastly because all these types, tragedy, comedy and melodrama, are usually grouped together as 'arts of the theatre' no less than as forms of 'literature.'

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[1] Cf. E. Bullough, 'The Perceptive Problem in the Aesthetic Appreciation of Single Colours,' this *Journal*, 1908, II. 406 ff.

From the point of view of the present discussion, the case of *comedy* is particularly involved. What we mean by comedy as a class of theatrical entertainment covers several different kinds[1], which actually merge into each other and present historically a continuity which allows of no sharp lines of demarcation (a difficulty, by the way, which besets all distinctions of literary or artistic *species*, as opposed to artistic *genera*). The second difficulty is that the 'laughable' includes much more than the comic of comedy. It may enter, in all its varieties of the ridiculous, silly, naïve, brilliant, especially as the humorous, into comedy as ingredients, but the comic is not coextensive with the laughable as a whole.

The fact to be noted here is, that the different types of comedy, as well as the different kinds of the laughable, presuppose different degrees of Distance. Their tendency is to have none at all. Both to laugh and to weep are direct expressions of a thoroughly practical nature, indicating almost always a concrete personal affection. Indeed, given suitable circumstances and adequate distancing-power, both can be distanced, but only with great difficulty; nor is it possible to decide which of the two offers the greater difficulty. The balance seems almost to incline in favour of tears as the easier of the two, and this would accord with the acknowledged difficulty of producing a really good comedy, or of maintaining a consistent aesthetic attitude in face of a comic situation. Certainly the tendency to *under*distance is more felt in comedy even than in tragedy; most types of the former presenting a *non-distanced,* practical and personal appeal, which precisely implies that their enjoyment is generally hedonic, not aesthetic. In its lower forms comedy consequently is a mere amusement and falls as little under the heading of Art as pamphleteering would be considered as *belles-lettres*, or a burglary as a dramatic performance. It may be spiritualised, polished and refined to the sharpness of a dagger-point or the subtlety of foil-play, but there still clings to it an atmosphere of amusement pure and simple, sometimes of a rude, often of a cruel kind.

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[1] Comedy embraces *satirical comedy*, i.e. dramatic invectives of all degrees of personal directness, from the attack on actually existing persons (such as is prohibited by the censorship, but has flourished everywhere) to skits upon existing professions, customs, evils, or society; secondly, *farce*, rarely unmixed with satire, but occasionally *pure* nonsense and horseplay; thirdly, *comedy proper*, a sublimation of farce into the pure comedy of general human situation, or genuine character-comedy, changing easily into the fourth class, the type of play described on the Continent as *drama* (in the narrower sense), i.e. a play involving serious situations, sometimes with tragic prospects, but having an happy, if often unexpected, ending.

This, together with the admitted preference of comedy for generalised types rather than for individualised figures, suggests the conclusion that its point of view is the survival of an attitude which the higher forms of Art have outgrown. It is noteworthy that this tendency decreases with every step towards high comedy, character-comedy and drama, with the growing spiritualisation of the comic elements and the first appearance of Distance. Historically the development has been slow and halting. There is no doubt that the 17th century considered the *Misanthrope* as amusing. We are nowadays less harsh and less socially intolerant and *Alceste* appears to us no longer as frankly ridiculous. The supreme achievement of comedy is unquestionably that 'distanced ridicule' which we call *humour*. This self-contradiction of smiling at what we love, displays, in the light vein, that same perfect and subtle balance of the 'antinomy of Distance' which the truly tragic shows in the serious mood. The tragic and the humorous are the genuine aesthetic opposites; the tragic and the comic are contradictory in the matter of Distance, as aesthetic and hedonic objects respectively.

A similar hedonic opposition in the other direction is to be found between tragedy and *melodrama*. Whereas comedy tends to *under*-distance, melodrama suffers from *over*distancing. For a cultivated audience its overcharged idealism, the crude opposition of vice and virtue, the exaggeration of its underlined moral, its innocence of *nuance*, and its sentimentality with violin-accompaniment are sufficient cause to stamp it as inferior Art. But perhaps its excessive distance is the least Distance obtainable by the public for which it is designed, and may be a great help to an unsophisticated audience in distancing the characters and events. For it is more than probable that we make a mistake in assuming an analogy between a cultivated audience at a serious drama, and a melodramatic audience. It is very likely that the lover of melodrama does not present that subtle balance of mind towards a play, implied in the 'antinomy of Distance.' His attitude is rather either that of a matter-of-fact adult or of a child: i.e. he is either in a frankly personal relation to the events of the play and would like to cudgel the villain who illtreats the innocent heroine, and rejoices loudly in his final defeat—just as he would in real life—or, he is completely lost in the excessive distance imposed by the work and watches naïvely the wonders he sees, as a child listens enchantedly to a fairy-tale. In neither case is his attitude aesthetic; in the one the object is *under*-, in the other *over*distanced; in the former he confuses it with the reality he *knows* (or thinks he knows) to exist, in the other with a reality whose existence he does *not know, but accepts.* Neither bears the twofold character of the aesthetic state in which *we know* a thing *not* to exist, but *accept its existence.* From the point of view of moral advantage—in the absence of any aesthetic advantage—the former attitude might seem preferable. But even this may be doubted; for if he believes what he sees in a great spectacular melodrama, every marble-lined hall of the most ordinary London hotel that he passes after the play must appear to him as a veritable Hell, and every man or woman in evening-dress as the devil incarnate. On either supposition, the moral effect must be deplorable in the extreme, and the melodrama is generally a much more fitting object of the censor's attention than any usually censored play. For in the one case the brutalising effect of the obtrusively visible wickedness cannot possibly be outweighed by any retaliatory poetic justice, which must seem to him singularly lacking in real life; in the other, the effect is purely negative and narcotic; in both his perspective of real life is hopelessly outfocussed and distorted.

2. The importance of Distance in artistic creation has already been briefly alluded to in connexion with the 'antinomy of Distance.'

Distancing might, indeed, well be considered as the especial and primary function of what is called the 'creative act' in artistic production: distancing is the *formal* aspect of creation in Art. The view that the artist 'copies nature' has already been dismissed. Since the 'imitation-of-nature' theory was officially discarded at the beginning of the 19th century, its place in popular fancy has been taken by the conception of the 'self-expression of the artist,' supported by the whole force of the Romantic Movement in Europe. Though true as a crude statement of the subjective origin of an artistic conception, though in many ways preferable to its predecessor and valuable as a corollary of such theories as that of the 'organic growth' of a work of Art, it is apt to lead to confusions and to one-sided inferences, to be found even in such deliberate and expert accounts of artistic production as that of Benedetto Croce[1]. For, to start with, the 'self-expression' of an artist is not such as the 'self-expression' of a letter-writer or a public speaker: it is not the *direct* expression of the concrete personality of the artist; it is not even an *indirect* expression of his concrete personality, in the sense in which, for instance, Hamlet's 'self-expression' might be supposed to be the indirect reflexion of Shakespeare's ideas. Such a denial, it might be argued, runs counter to the observation that in the works of a literary artist, for example, are to be found echoes and mirrorings of his times and of his personal experiences and convictions. But it is to be noted that to find these *is* in fact impossible, unless you previously know what reflexions to look for. Even in the relatively most direct transference from personal experiences to their expression, viz. in lyrical poetry, such a connexion cannot be established backwards, though it is easy enough to prove it forwards: i.e. given the knowledge of the experiences, there is no difficulty in tracing their echoes, but it is impossible to infer biographical data of any detail or concrete value from an author's works alone. Otherwise Shakespeare's *Sonnets* would not have proved as refractory to biographical research as they have done, and endless blunders in literary history would never have been committed. What proves so impossible in literature, which after all offers an exceptionally adequate medium to 'self-expression,' is *a fortiori* out of question in other arts, in which there is not even an equivalence between the personal experiences and the material in which they are supposed to be formulated. The fundamental two-fold error of the 'self-expression' theory is to speak of 'expression' in the sense of 'intentional communication,' and to identify straightway the √ artist and the man. An intentional communication is as far almost from the mind of the true artist as it would be from that of the ordinary respectable citizen to walk about naked in the streets, and the idea has repeatedly been indignantly repudiated by artists. The second confusion is as misleading in its theoretical consequences, as it is mischievous and often exceedingly painful to the 'man' as well as to the 'artist.' The numberless instances in history of the astonishing difference, often the marked contrast between the *man* and his *work* is one of the most disconcerting riddles of Art, and should serve as a manifest warning against the popular illusion of finding the 'artist's mind' in his productions[1].

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[1] Benedetto Croce, *Aesthetic*, translated by Douglas Ainslie, Macmillan, 1909.

Apart from the complication of technical necessities, of conventional art-forms, of the requirements of unification and composition, all impeding the direct transference of an actual mental content into its artistic formulation, there is the interpolation of Distance which stands between the artist's conception and the man's. For the 'artist' himself is already distanced from the concrete, historical personality, who ate and drank and slept and did the ordinary business of life. No doubt here also are *degrees* of Distance, and the 'antinomy' applies to this case too.

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[1] Some well-known examples of this difference are, for instance: Mozart, Beethoven, Watteau, Murillo, Molière, Schiller, Verlaine, Zola.

Some figures in literature and other arts are unquestionably self-portraits; but even self-portraits are not, and cannot be, the direct and faithful cast taken from the living soul. In short, so far from being 'self-expression,' *artistic production is the indirect formulation of a distanced mental content.*

I give a short illustration of this fact. A well-known dramatist described to me the process of production as taking place in his case in some such way as follows:

The starting-point of his production is what he described as an 'emotional idea,' i.e. some more or less general conception carrying with it a strong emotional tone. This idea may be suggested by an actual experience; anyhow the idea itself *is* an actual experience, i.e. it occurs within the range of his normal, practical being. Gradually it condenses itself into a situation made up of the interplay of certain characters, which may be of partly objective, partly imaginative descent. Then ensues what he described as a "life and death struggle" between the idea and the characters for existence: if the idea gains the upper hand, the conception of the whole is doomed. In the successful issue, on the contrary, the idea is, to use his phrase, "sucked up" by the characters as a sponge sucks up water, until no trace of the idea is left outside the characters. It is a process, which, he assured me, he is quite powerless to direct or even to influence. It is further of interest to notice that during this period the idea undergoes sometimes profound, often wholesale changes. Once the stage of complete fusion of the idea with the characters is reached, the conscious elaboration of the play can proceed. What follows after this, is of no further interest in this connexion.

This account tallies closely with the procedure which numerous dramatists are known to have followed. It forms a definite type. There are other types, equally well supported by evidence, which proceed along much less definite lines of a semi-logical development, but rather show sudden flash-like illuminations and much more subconscious growth.

The point to notice is the "life and death struggle" between the idea and the characters. As I first remarked, the idea is the '*man's*,' it is the reflexion of the dramatist's concrete and practical self. Yet this is precisely the part which must "die." The paradox of just the germ-part of the whole being doomed, particularly impressed my informant as a kind of life-tragedy. The 'characters' on the other hand belong to the imaginary world, to the 'artist's.' Though they may be partially suggested by actuality, their full-grown development is divorced from it. This process of the 'idea' being "sucked up" by the characters and being destroyed by it, is a phase of artistic production technically known as the 'objectivation' of the conception. In it the 'man' dies and the 'artist' comes to life, and with him the work of Art. It is a change of death and birth in which there is no overlapping of the lives of parent and child. The result is the distanced finished production. As elsewhere, the distancing means the separation of personal affections, whether idea or complex experience, from the concrete personality of the experiencer, its filtering by the extrusion of its personal aspects, the throwing out of gear of its personal potency and significance.

The same transformation through distance is to be noticed in *acting*. Here, even more than in the other arts, a lingering bias in favour of the 'imitation of nature' theory has stood in the way of a correct interpretation of the facts. Yet acting supplies in this and other respects exceptionally valuable information, owing to its medium of expression and the overlapping—at least in part—of the process of producing with the finished production, which elsewhere are separated in point of time. It illustrates, as no other art can, the cleavage between the concrete, normal person and the distanced personality. [The acting here referred to is, of course, not that style which consists in 'walking on.' What is meant here is 'creative' acting, which in its turn must be distinguished from 'reproductive' acting—two different types traceable through the greater part of theatrical history, which in their highest development are often outwardly indistinguishable, but nevertheless retain traces of differences, characteristic of their procedures and psychical mechanism.] This cleavage between the two streams or layers of consciousness is so obvious that it has led to increasing speculation from the time when acting first attracted intelligent interest, since the middle of the 18th century. From the time of Diderot's *Paradoxe sur le Comédien* (itself only the last of a series of French studies) down to Mr William Archer's *Masks or Faces* (1888) and the controversy between Coquelin and Salvini (in the nineties), theory has been at pains to grapple with this phenomenon. Explanations have differed widely, going from the one extreme of an identification of the acting and the normal personality to the other of a separation so wide as to be theoretically inconceivable and contradicted by experience. It is necessary to offer some conception which will account for the differences as well as for the indirect connexion between the two forms of being, and which is applicable not merely to acting, but to other kinds of art as well. Distance, it is here contended, meets the requirement even in its subtlest shades. To show this in detail lies outside the scope of this essay, and forms rather the task of a special treatment of the psychology of acting.

[3. In the interest of those who may be familiar with the developments of aesthetic theories of late years, I should like to add that Distance has a special bearing upon many points raised by them. It is essential to the occurrence and working of 'empathy' (*Einfühlung*), and I mentioned earlier its connexion with Witasek's theory of *Scheingefühle* which forms part of his view on 'empathy.' The distinction between sympathy and 'empathy' as formulated by Lipps[1] is a matter of the relative degree of Distance. Volkelt's[2] suggestion of regarding the ordinary apprehension of expression (say of a person's face) as the first rudimentary stage of *Einfühlung*, leading subsequently to the lowering of our consciousness of reality ("*Herabsetzung des Wirklichkeitsgefühls*"), can similarly be formulated in terms of Distance. K. Lange's[3] account of aesthetic experience in the form of 'illusion as conscious self-deception' appears to me a wrong formulation of the facts expressed by Distance. Lange's 'illusion' theory seems to me, among other things[4], to be based upon a false opposition between Art and reality (nature) as the subject-matter of the former, whereas Distance does not imply any comparison between them in the act of experiencing and removes altogether the centre of gravity of the formula from the opposition.]

4. In this way Distance represents in aesthetic appreciation as well as in artistic production a quality inherent in the impersonal, yet *so* intensely personal, relation which the human being entertains with Art, either as mere beholder or as producing artist.

It is Distance which makes the aesthetic object 'an end in itself.' It is that which raises Art beyond the narrow sphere of individual interest and imparts to it that 'postulating' character which the idealistic philosophy of the 19th century regarded as a metaphysical necessity. It renders questions of origin, of influences, or of purposes almost as meaningless as those of marketable value, of pleasure, even of moral importance, since it lifts the work of Art out of the realm of practical systems and ends.

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[1] Th. Lipps, *Aesthetik*, Hamburg and Leipzig, 1903, I.; 'Aesthetische Einfühlung,' *Ztsch. für Psychol. u. Physiol. der Sinnesorg.* XXII. 415 ff.

[2] J. Volkelt, *System der Aesthetik*, 1905, I. 217 ff. and 488 ff.

[3] K. Lange, *Des Wesen der Kunst*, 1901, 2 vols.

[4] J. Segal, 'Die bewusste Selbsttäuschung als Kern des aesthetischen Geniessens *Arch. f. d. ges. Psychol.* VI. 254 ff.

In particular, it is Distance, which supplies one of the special criteria of aesthetic values as distinct from practical (utilitarian), scientific, or social (ethical) values. All these are concrete values, either *directly* personal as utilitarian, or *indirectly* remotely personal, as moral values. To speak, therefore, of the 'pleasure value' of Art, and to introduce hedonism into aesthetic speculation, is even more irrelevant than to speak of moral hedonism in Ethics. Aesthetic hedonism is a compromise. It is the attempt to reconcile for public use utilitarian ends with aesthetic values. Hedonism, as a practical, personal appeal has no place in the distanced appeal of Art. Moral hedonism is even more to the point than aesthetic hedonism, since ethical values, *quâ* social values, lie on the line of prolongation of utilitarian ends, sublimating indeed the *directly* personal object into the realm of socially or universally valuable ends, often demanding the sacrifice of individual happiness, but losing neither its *practical* nor even its *remotely personal* character.

In so far, Distance becomes one of the distinguishing features of the 'aesthetic consciousness,' of that special mentality or outlook upon experience and life, which, as I said at the outset, leads in its most pregnant and most fully developed form, both appreciatively and productively, to Art.

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SOME OBSERVATIONS CONCERNING COLOUR VISION IN DOGS[1].

BY E. M. SMITH.

(*From the Cambridge Psychological Laboratory.*)

I. *Aim of the present research.*
II. *Summary of previous experiments.*
III. *Description of the apparatus here used; precautions to insure the elimination of accidental factors as possible determinants of discrimination.*
IV. *Description of the method and of the order of series; system of marks; graded and percentage methods; reward and punishment; advantages and disadvantages of the method.*
V. *Results of the various series.*
VI. *Specific effects of the primary colours.*
VII. *Summary of results: A. evidence in support of the view that dogs are only able to perceive differences of brightness; B. evidence in support of the view that dogs possess rudimentary colour discrimination.*
VIII. *The effects of practice; qualitative and quantitative improvement; transference of improvement; daily improvement; asymptotic character of improvement; deterioration with practice.*
IX. *Retentiveness.*
X. *Description of behaviour and growth of discrimination; requisite conditions of a sensory discrimination test; nature of the mean variation in sensory discrimination tests on the higher vertebrates. The signs of recognition. Reaction types (a) affirmative, (b) negative. Method of comparison. Position errors (a) absolute, (b) relative, forms. Self-control. Inhibition. Conditions serving (a) to mask, (b) to aid, discrimination. Habit formation and relative plasticity.*
XI. *The rôle of kinaesthetic sensations and the position sense.*
XII. *Individual differences.*
XIII. *General conclusions.*
Appendix.—Tables.

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[1] These observations were made in the Cambridge Psychological Laboratory during the experimenter's tenure of a Marion Kennedy Studentship granted by Newnham College.

I. AIM OF THE PRESENT RESEARCH.

THE endeavour to replace, in the field of animal psychology, the older methods of anecdote and chance observation by those laying claim to greater scientific exactitude has already yielded a vast mass of interesting results. The examination of an animal's sensory experience and of the degree to which its power of sensory discrimination admits of development by means of careful training is now recognised as an indispensable preliminary to any analysis of the higher 'mental' processes of that animal. Accordingly we find that the greater part of recent investigations in animal psychology consists in a detailed examination of the ability of different species of animals to respond to various sensory stimuli. Vision has perhaps received the chief share of attention; this is probably due to a tacit assumption that throughout the animal kingdom its *rôle* is everywhere as important for the exercise of the higher mental functions as it is in the case of man himself. Following this line of argument we should be led to expect that in such an animal as the dog, which for countless years has been man's constant companion, the sense of vision would have received its full share of attention. But, strangely enough, despite the apparent suitability of dogs as subjects for psychological experiment, despite their accessibility, docility, diversity of breed, and despite the intelligence popularly ascribed to them, singularly little work of an exact nature has been recorded concerning their behaviour; and when we come to consider the particular question of the ability of dogs to perceive colours, we find that up to the present time the data available are insufficient to justify any definite pronouncement. This is in great part due to the fact that in the majority of cases the work recorded and the methods employed have not been rigorous enough in character. It therefore seemed important to make further observations by a more exhaustive method.

Thus the aim of the experiments to be described in this paper is to discover whether dogs are able to perceive differences in hue as distinguished from mere differences in brightness; and, should this be answered in the affirmative, to determine, as far as may be, the nature of such colour vision, and the use made of it by these animals.

I should here like to express my thanks to Dr Myers, under whose direction the present inquiry was conducted, for his suggestion of the subject as well as of the method of experimentation, and my indebtedness to him for constant help and advice.

II. SUMMARY OF PREVIOUS EXPERIMENTS.

A brief summary of the previous work dealing with the colour vision of dogs, preparatory to an account of the observations forming the subject-matter of this paper, will serve to make clear the present position of our knowledge in this field.

Common to all the experiments is the very small number of subjects employed, one or two being the usual number and seldom more than three (table A, page 129).

Pursuing very diverse methods, previous investigators have arrived at contradictory results. We find on the one side Lubbock, Orbeli and Nicolai declaring that their observations fail to yield any evidence in favour of the view that dogs are able to discriminate between light waves of different lengths; whereas on the other side, V. Graber, E. Gates, Nagel and Himstedt, Samojloff and Pheophilaktowa, Colvin and Burford, and Kalischer all maintain that dogs can, and to some extent do, distinguish between various colours. The work of these observers falls into three groups according as:

1. the discrimination of hue is confused with that of brightness;
2. a distinction is made between hue and brightness discrimination, but the brightness values of the various colours are assumed to be the same for dogs as for man;
3. no such assumption is made; the brightness values of the colours employed being determined for dogs irrespectively of the values they may possess for man.

Considering the work in greater detail, we may first examine those observations from which negative conclusions have been drawn.

In 1888 Lubbock[1] experimented with one poodle. The method employed was as follows. Three pairs of cards coloured respectively blue, orange and yellow were used. One card from each pair was placed on the floor, the position being varied irregularly. The remaining card of one of the pairs was then shown to the dog in the hope that, after training, she would learn to select from the three cards before her the one that was of the same colour as that held up by her master. Training was continued for ten weeks, but entirely without success, although the dog had proved her ability to make use of unusual reactions in previous and somewhat similar tests. Lubbock remarks, however, that he does not consider these observations to be conclusive, as the animal seemed to have no idea of what was required. Nevertheless they serve to show the difficulty experienced by an intelligent dog in picking out either colour or brightness as the important or distinguishing mark in situations otherwise similar—a fact which would be surprising, did colour discrimination play an active part in the dog's 'mental' life.

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[1] *The Senses, Instinct and Intelligence of Animals*, London, 1888, 280–81.

In 1908 Orbeli[1] in an extensive investigation of Pawlow's 'conditioned' salivary reflexes resulting from visual stimuli, found no evidence that his subjects were able to discriminate between different colours, though their discrimination of different luminosities was fairly delicate. It is noteworthy that Orbeli carefully distinguished the problem of colour from that of mere brightness perception. Three subjects were tested, yielding the following results:

Test 1. Subject B was trained to red as food-colour. After training it was found that the appearance on the screen[2], which was unilluminated in the interval, of green, blue, violet or white light proved as effective as the food-colour itself in producing the conditioned reflex. Further, the waning of the reflex brought about by successive exposures of blue, green, violet or white induced an equal waning of the food-colour reflex. Orbeli attributes this result to the fact that light alone determines the reaction so long as such light can be discriminated.

Test 2. Subject B. Only red and green lights were used, the screen again remaining unilluminated in the interval. At the end of one and a half months no trace of any ability to discriminate between the two colours was discovered (green 82, red 78). Any changes which occurred were found to hold good for both colours in the same manner and degree.

Test 3. Subject B. The screen was illuminated throughout, green being always before the subject, except when replaced by the food-colour, red. After 140—145 excitations there were signs of the reflex, which was however exceedingly weak and inconstant, and proved equally effective whatever the colour presented—*e.g.* violet, blue, yellow, etc. Orbeli explains this result as being due to the possible insignificant change in brightness when green is replaced by any of the other colours.

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[1] "Réflexes conditionnels du côté de l'œil chez le chien," *Archives des Sciences Biologiques*, 1909, xiv. Nos. 1 and 2.

I should like to take this opportunity of thanking Dr Orbeli for his kindness in explaining to me in detail his own experiments, and for the interest which he took in the present observations.

[2] Orbeli's method of presenting the various colours to his subjects was to throw light of the colour required on to a screen placed in front of the animal.

Test 4. Subjects G and K. A white screen always before the subject except when illuminated by a green light or by the food-colour, red. With subject G there was no trace of the formation of a reflex when red had been presented 95, and green 215, times. And for subject K any reflex formed worked in precisely the same direction for both colours, the same holding good as regards the extinction of the reflex (121 excitations). In further tests blue was substituted for green, yielding a similar result.

Test 5. Subject K. A white screen, occasionally illuminated by the food-colour, red, was before the subject. A definite reflex appeared after 10—15 excitations. When the light was cut off, or colours other than the food-colour were presented, a flow of saliva was produced equal in amount to that obtained upon excitation by the food-colour. Orbeli concludes that the reflex thus obtained is due solely to the diminution in brightness which occurs when white is replaced by a colour. Additional evidence in support of this assumption is afforded by the fact that when red was permanently presented to the subject there was no salivary flow, whereas, when the brightness was diminished, a strong reflex at once appeared.

From the above experiments Orbeli concludes that the reactions of the dogs were determined throughout by changes in brightness and that no evidence of their ability to discriminate between colours, as such, is shown.

Certain criticisms, however, may be made of these conclusions. In the first place we shall see reason to believe that, even in dogs that *do* show ability to perceive colours, such colour perception is very unstable and can only be manifested under the most favourable conditions, impressions received from other simultaneous stimuli readily inhibiting those due to colour stimuli (pages 188, 189). Moreover, we shall show that the 'absolute' method (*i.e.* where the colours are presented successively and thus admit of indirect comparison only) is distinctly unfavourable to positive results, the subject needing, with this method, an extremely long period of training before any trace of colour perception is revealed (page 181). Further, it is possible that the length of training given was not long enough to allow of the subjects analysing, out of the fundamental fact of *change*, the significance of the part played by colour; they appeared always to react to the mere fact of change. In this respect, a suggestive feature is to be noted in test 1. There the appearance of white occasioned exactly the same response as that produced by the colours.

That is to say, in spite of the relatively fine ability shown by the dogs in previous tests to discriminate readily between different degrees of brightness, in this particular test a presumably large difference in brightness proved ineffective.

Nicolai[1], writing in 1907, also denies the ability of dogs to discriminate between colours. He presented to his subjects green and red vessels. In order to eliminate the possibility of any apparent discrimination being due to the perception of a constant difference in brightness, the illumination of the vessels was frequently varied by definite degrees—the result being that the two animals employed were utterly puzzled and quite uncertain in their choice.

When we turn to the consideration of the evidence offered in favour of the view that dogs are able to discriminate between different colours apart from any difference in brightness, we find the data at our command to be extremely slight.

The earliest observations recorded are those made by Vitus Graber[2] in 1884. The procedure adopted was that of his well-known 'preference' method. Seven of the dogs tested were found to be indifferent to considerable differences in light and colour. But two others, a poodle and a fox-terrier, gave evidence of definite and decided preferences. A previous control test seemed to indicate that the position-error was negligible. The following results were then obtained. When black and white formed the contrasting pair, the number of times that white was chosen by the two subjects was fifty-six, whereas black was only selected four times. With light red and a darkish blue, the latter was preferred eighty-three, the former seven, times. The same red presented with a much darker blue gave red 28, blue 32. From these observations Graber deduces that the dogs tested were 'cyanophile' and 'erythrobe,' but that as the brightness of the blue was diminished the preference manifested for it disappeared. These results are inconclusive as regards colour vision and certainly do not warrant the above deduction. It is quite in accordance with the data to assume that the selections made were based entirely on brightness differences, the subjects always preferring the brighter to the less bright.

Some experiments were recorded by Elmer Gates[3] in 1895. Unfortunately the description given is so meagre that it is impossible to ascertain whether precautions were taken to distinguish colour from mere brightness discrimination. Even the astounding fact that seven dogs learnt to discriminate between seven shades of red, "not purple and red" (this is significant) is scarcely conclusive. And in earlier experiments, where the subjects had to pick out on a pavement composed of variously coloured metal squares a path of the same colour as the square from which they started, it is by no means certain that the eventual learning of the path may not have been due to position memory alone, since we are not told that the position of the squares was ever changed.

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[1] "Die physiologische Methodik zur Erforschung der Tierpsyche, ihre Möglichkeit und ihre Anwendung," *J. f. Psychol. u. Neur.* 1907, x. Heft 1/2.

[2] *Grundlinien zur Erforschung des Helligkeits- und Farbensinnes der Thiere*, Prag, 1884.

[3] "The Science of Mentation," *The Monist*, 1895, v. 574–97.

Nagel[1] and Himstedt[2], experimenting on a poodle, taught it at the command "Bring red" to select the red from among a number of balls of various hues. Later, when several reddish balls were placed with the others, the dog on command would bring them to his master in the order of their redness, the red ball being chosen first, strawberry next, then orange and so on. Nagel and Himstedt were careful to distinguish the quantitative from the qualitative aspect of the question. But though they affirm that differences of luminosity are of prior importance to the dogs, they claim further that they have obtained definite reactions of a determinate character even in the absence of any difference of brightness. Nevertheless we are not told how Nagel satisfied himself that no difference in brightness existed for his subjects.

Samojloff and Pheophilaktowa[3] endeavoured to find out as exactly as possible the brightness value *for the subject* of the colour (green) used in their experiments. Their chief aim was to discover whether, in the absence of any difference in brightness, dogs were able to discriminate between light waves of different lengths. But discrimination between a saturated green and a series of graduated greys proving so difficult in the case of the one subject tested, the inquiry was changed in form, the new problem posed being, whether a dog can be taught *by much training* to distinguish between different colours as well as between different brightnesses. According to the method adopted, three boxes placed in a straight row 40 cm. apart were presented to the subject. To the front of two of the boxes were attached discs, 25 cm. each in diameter, cut from Nendel's grey papers; to the third, a saturated green disc of the same dimensions. The position of the boxes was changed in irregular order after every test. The dog was placed five metres distant, and required to select from the three boxes the one bearing the green disc, under which biscuit was concealed. After training it was found that out of fifty papers ranging from black to white, there was no grey which, when presented to the subject for discrimination from the standard green, yielded a percentage of errors larger than 31. But later tests revealed that such discrimination only held good under certain conditions. The green figure was given irregularly in one of the three forms, circle, triangle or square, the grey always being presented as a square. Under these conditions the subject chose the green circle readily enough, but when the green appeared in one of the two novel and unfamiliar forms he invariably preferred the grey square. The deduction drawn is that discrimination of a colour from a series of greys is only possible where the conditions are well known; on the introduction of any change some factor other than colour tends to become the guiding sign.

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[1] "Der Farbensinn des Hundes," *Zentbl. f. Physiol.* 1907, xxi. 205–6.

[2] "Versuche über die Reizwirkung verschiedener Strahlen-arten auf Menschen- und Tieraugen," *Festschrift des Albrecht-Ludwigs-Universität in Freiburg*, 1902.

[3] "Ueber die Farbenwahrnehmung beim Hunde," *Zentbl. f. Physiol.* 1907, xxi. 133–9.

Some dogs tested by Colvin and Burford[1] gave evidence of a surprisingly high degree of colour discrimination. The experiments, which extended over a period of ten months, fall into three groups. In the first group the object was to discover whether the dogs could learn to distinguish a certain red, used as 'standard,' from several other colours including red-orange, red-red-orange and red no. 2. Painted boxes, alike in size and shape, differing only in colour, were again employed. Food could be obtained from the standard red box by turning a small latch, and then pulling or pushing at the door so released. Similar latches on the other boxes were securely fastened by a nail. This arrangement admits of a possible though not probable source of error, inasmuch as the subjects may have reacted merely to the presence or absence of the nail.

For subject 1 the experiments extended over a period of 158 days, during which time he successfully learnt to distinguish all the eight colours and a medium grey of the same brightness as the colours, from the red standard box. The two cases which presented the greatest degree of difficulty were grey (66·7 °/₀) and red no. 2 (64·4 °/₀); the percentages of successful discriminations for the other colours were all high, not falling below 84 °/₀ except in the case of orange (77·5 °/₀). Subject 2 showed the lowest degree of discrimination (54·8 °/₀) for red no. 2; 67·7 °/₀ was obtained for green, the percentages for the remaining colours ranging from 88 °/₀ to 77 °/₀. Apparently grey was not presented.

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[1] "The Colour Perception of three Dogs, a Cat and a Squirrel," *Psychol. Rev., Monogr. Suppl.* 1909, XI. 1–44.

The results obtained for the third subject were very indecisive and practically no evidence of discriminative ability was obtained.

The experimenters, in order to eliminate differences of brightness, matched the various colours used with a medium grey. They admit however that "the fact that these colours appeared of equal intensity to the human eye does not necessarily mean that they have the same brightness value for all animals[1]." In this connexion it is to be noticed "that the red-orange was more successfully discriminated from the standard red than was the blue, yellow, green or violet"; also that according to one method of tabulation "even red-orange stands above yellow, violet and green and is practically on a par with blue[2]," whereas the least discrimination is shown for red no. 2. The possibility suggests itself that the mixture of the red and orange pigments resulted in some change in brightness for the subjects, for it is difficult to explain on purely qualitative grounds the fact that green should have proved so much more difficult than the red-oranges to distinguish from the standard red.

In the second group of tests subject 2 alone was examined. Green was substituted for the original red as standard, and the dog was required to select it from the remaining colours which were presented successively. The process was then repeated with blue as standard. The experiment was continued for nine weeks, during which a high degree of discrimination was shown.

The original food-colour was again the standard in the third group of experiments, but this time it was presented under different forms and conditions. A lengthy period of training was required before any degree of accuracy was attained, but eventually subjects 1 and 3 gave distinct evidence of ability to single out red from the other colours under the novel conditions.

It is to be regretted that Colvin and Burford have only expressed their results as percentages, and that they record neither the relative length of time required to learn a colour, nor the total numbers of successful choices considered necessary to establish proof of discrimination in a given case.

Experiments yielding interesting results have been described by Kalischer[3]. His animals were placed in a dark chamber which could be illuminated by means of light from a lamp, transmitted through coloured glass. On the appearance of red light the dog was given food, which was, however, withheld during the period when light was excluded. When a perfect reaction habit had been formed, the experiment was varied sometimes by transmitting the light through blue, instead of through red, glass, though the subject was still only fed when a red light was exposed. In order to render the comparison of brightness between the different pairs of lights more difficult, their successive exposure was always followed by a dark interval. At first the subjects reacted in a similar manner to either coloured light, but after some further training the difference was duly learnt, and eventually discrimination became so perfect that, no matter how the brightness of the red or the blue light might be changed, the reaction was almost invariably correct.

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[1] *Ibid.* 18.

[2] *Ibid.* 42.

[3] "Weitere Mitteilungen über die Ergebnisse der Dressur als physiologische Untersuchungsmethode auf den Gebieten des Gehör-, Geruchs-, und Farbensinns," *Arch. f. Anat. u. Physiol.* (Physiol. Abt.). 1909, 316–22.

Observations were also made with green, yellow and red-violet lights, but discrimination was found to be the most definite when red and blue were used; red and yellow proving very difficult to learn, and red with red-violet almost impossible. Colvin and Burford likewise found that when their subjects were required to discriminate between red and violet, only a relatively low percentage of right choices was obtained.

In later experiments Kalischer would leave his subjects untested for two or three days, and then present a blue or a green light previous to the exposure of the food-colour, red. Though these experiments were repeated many times and under varied conditions, the animals never failed to react correctly to the food-colour when it appeared.

It is of importance to note that not only did the animals learn to discriminate, during the course of the experiments, between different colours irrespectively of the degree of their luminosity, but that in addition the behaviour shown on the appearance of each colour was characteristic; *e.g.* on the exposure of a blue light the subject would appear frightened and quickly withdraw his head from the food; whereas when a green or a yellow light was exposed, great hesitation was shown, the head was allowed to fall gradually nearer and nearer towards the meat as though to seize it, and was then suddenly jerked away.

The conclusions which Kalischer draws from his experiments are as follows:—

(*a*) That there is no doubt of the ability of dogs to perceive differences of hue as well as differences of brightness.

(*b*) That whereas such colour discrimination is cortical, brightness discrimination is to some extent sub-cortical, reactions to changes of illumination being found to occur even after extirpation of the visual cortex.

(*c*) That there exist considerable individual differences in the sensibility of dogs to colours.

From this brief summary of previous work it will be seen that all previous observers are in agreement as to the difficulty experienced by dogs in discriminating between light-waves of different lengths. The divergence in the results of these observers may be attributed in part to

(i) an inadequate analysis of the conditions involved,
(ii) the diverse methods employed,
(iii) the variety of breeds from which the subjects were selected,
(iv) individual differences in the adaptability of the subjects to the external conditions of the experiment.

The following table gives a synopsis of the number, breed, age, etc. of the dogs used by various experimenters (including those tested in the present research) and the conclusions—affirmative or negative—of these experimenters in regard to the colour vision of dogs.

TABLE A.

Experimenter	Breed and age	Sex	Nature of the results obtained
Lubbock	Poodle	♀	Negative
Orbeli	Yard-dog (B)[1]	♀	
	Mongrel (K) (hunting × yard)	♀	
	Gordon setter (G)	♂	
Nicolai	2 dogs (breed and age ?)	?	
Graber	Pomeranian	♂	No results
	4 Fox-terrier mongrels	?	
	Pointer	♂	
	Scotch-terrier	♂	
	Fox-terrier	♂	Affirmative
	Poodle	♂	
Gates	7 Shepherd pups	?	
Nagel and Himstedt	Poodle	♂	
Samojloff and Pheophilaktowa	One dog (breed and age ?)	♂	
Colvin and Burford	3 dogs (breed and age ?)	♂ ♂ ♂	
Kalischer	2 Mongrels (aged 1 to 2 yrs.)	♂ ♂	
Smith	Fox-terrier (J), aged 5 yrs.	♂	
	Welsh terrier mongrel (T), aged 14 months	♂	
	Whippet (R), aged 19 months	♀	
	Schipperke, aged 2 yrs.	♀	No results; subjects unsuited to the conditions of the experiment
	Irish terrier, aged 10 months	♂	
	2 Fox-terriers (B and G), aged 1 yr. and 4 yrs.	♂ ♂	

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[1] The letters in brackets placed after the breed of dog in the above table indicate the individual dogs to which reference is made in the course of this paper.

III. DESCRIPTION OF THE APPARATUS HERE USED.

The apparatus employed in the experiments forming the subject-matter of this paper was exceedingly simple. It consisted of a large deal case *ABCD* (fig. 1), measuring 8 ft. in length, by 6 ft. 4 in. to 5 ft. 4 in. in breadth and 2 ft. in height. This case was divided into two compartments by a screen *EF*, which carried five shutters; the smaller compartment, or home-box, was 3 ft. 3 in. × 5 ft. 4 in., the larger, or trial-box, 4 ft. 8 in. × 6 ft. 4 in. The home-box was only used as an ante-chamber to the trial-box. It was filled with straw, and admission could be gained by doors, 2 ft. 2 in. × 1 ft. 10 in., situated at either end, which served as a means of entrance and exit for the dogs, and enabled the experimenter to reach the shutters when changing the cards. The whole case was covered in with wire-netting (fig. 2), in order to prevent the animals escaping by the top of the box[1], as at first this possibility in every case proved a very distracting factor. The screen, *EF*, carrying the shutters, S_1, S_2, S_3, S_4 and S_5, stood on a glass and wood base (fig. 2), 6½ in. high, 1 in. in width, and

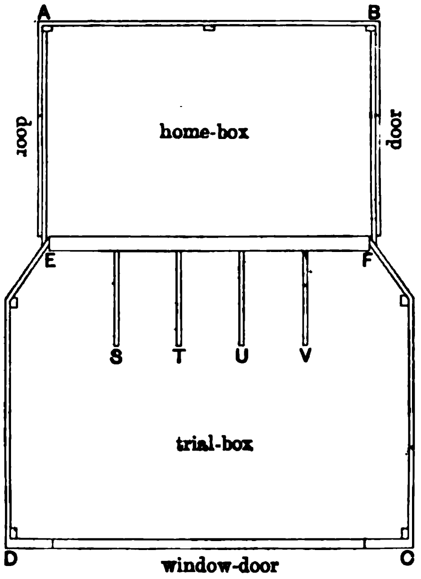

FIG. 1.

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[1] The total height of the case from the floor to the highest point of the wire-netting measured 3 ft.

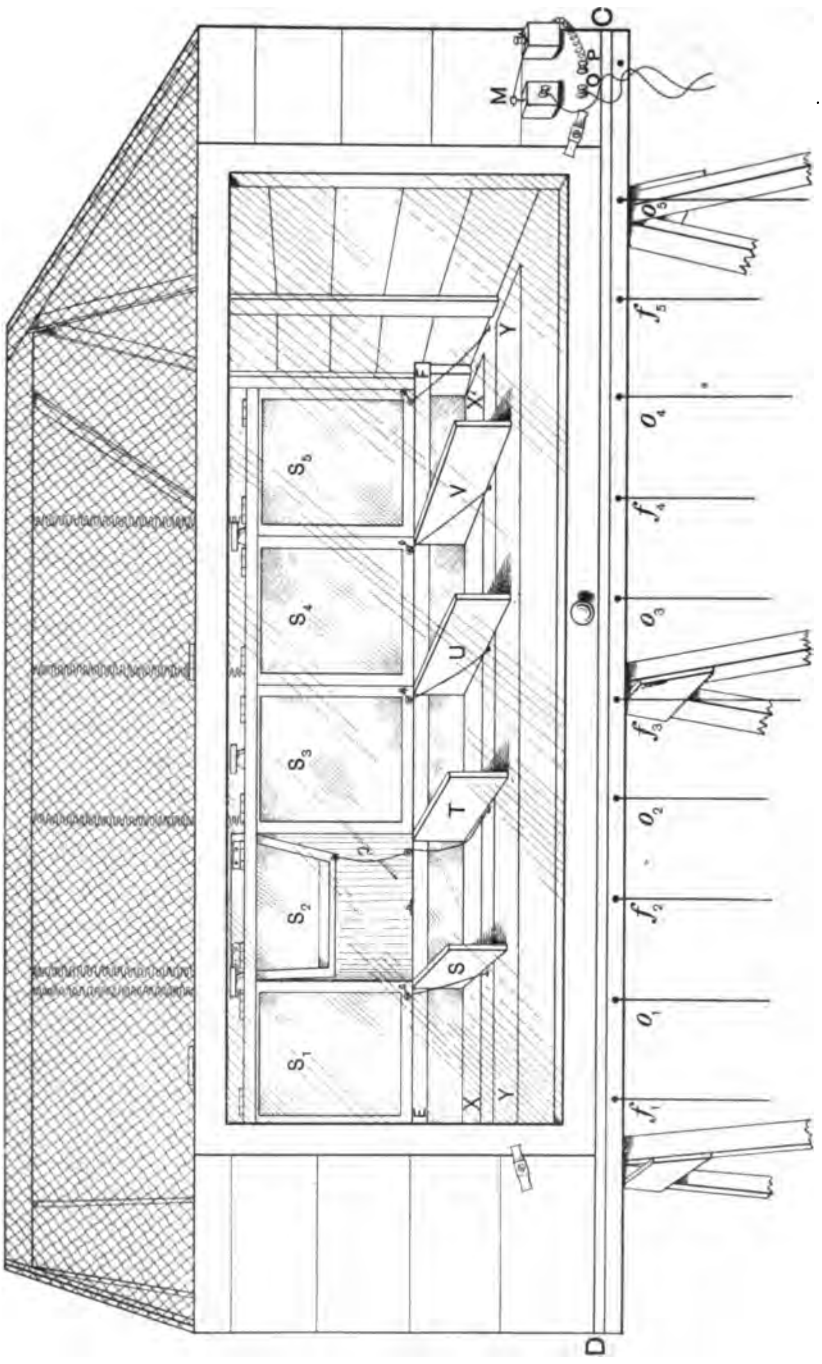

FIG. 2.

extending the full breadth of the case. Each shutter consisted of a wooden frame, 1 ft. 2 in. × 11 in. (*i.e.* of sufficient size for a large fox-terrier to pass through without difficulty), containing a glass window 1 ft. × 9 in. The frame had a groove in either side through which a card could be slipped behind the glass, the card then being retained in position by the wooden frame-work. The shutters (immediately adjacent to one another) opened upwards and inwards towards the home-box; they were opened and closed by means of springs. One string (*f*) was attached to the lower spring, which when taut kept the shutter closed, another string (*o*) to the shutter itself; these strings then passed through the floor of the case, continuing along it underneath, and reappearing through holes pierced in a board nailed below the front (*CD*) of the case where the operator stood during the experiments. In order to manipulate the shutters as desired, it was sufficient to pull either the 'open' (*f*) or the 'shut' (*o*) string attached to the particular shutter in question. But the operator never touched the strings until the dog had made its choice and settled before a shutter, lest the animal should learn to form an association between the different strings and certain shutters.

The front (*CD*) of the trial-box contained a large door opening upwards and outwards, in the frame-work of which was inserted a window, 4 ft. 5 in. × 1 ft. 6 in., which served to transmit light directly to the shutters and at the same time enabled the experimenter to observe all the dogs' movements without coming into contact with the animals. During discrimination the subject had his back to the experimenter and was about 2½ ft. distant from the window-door.

At the point of junction of each shutter with that adjacent to it, small wooden divisions, *S*, *T*, *U*, *V*, 1 ft. 4 in. in length, ran out into the trial-box; these were of the same height as the base supporting the shutter-screen and about 1 in. in width; their function was to demarcate clearly the subject's selection of a particular shutter. The dogs were speedily taught that, however long and earnestly they might regard a card contained in one of the shutters, they would, nevertheless, not be allowed to pass through to the home-box until they had entered between the partitions leading directly to the shutter in question and unequivocally regarded the selected card; and it was found that this device was indispensable if any degree of exactitude were to be attained.

A glance at fig. 1 will show that the sides of the trial-box sloped outwards from the screen *EF*, making with it an angle of 125°. This was done in order to render the illumination of the various shutters as even as possible, and to prevent that of the two end ones being diminished in intensity by any shadows which the sides of the case might cast; also to enable the dogs to obtain a full view of these shutters without necessarily coming into contact with the aforesaid sides.

Lastly, two metal plates, *XX*, *YY*, were fixed to the floor of the trial-box, near to the shutter-screen and extending the full width of the case (fig. 2). The plates were each 1 ft. in breadth, and were separated by a distance of 2 in. To either plate was attached a wire; the wires passed inside the case and were attached at the front to *O* and *P*. *O* and *M* were in direct connexion with the secondary circuit of an induction coil. At *M* was a lever which could be depressed by the operator into a small bowl of mercury. The circuit was completed and the shock given when the animal stood in such a position that it had one of its paws on *X* and another on *Y*.

The case was placed on a stand (fig. 2) at about 3 ft. from the floor, and on a level with the windows which faced almost due south, the side *DC* being parallel and near to, while the side *AB* was distant from, the windows. With the exception of certain 'training series' and very bright days, the experiments always took place in the morning between 9.30 a.m. and 1 p.m.

The tests throughout were invariably by means of reflected, and never transmitted, light. For the observations on brightness sensibility R. Nendel's series of grey papers was employed[1]. Of the different colours used, red blue, green, yellow, magenta and orange, the most saturated (hereafter styled 'max.') was in each case selected from Zimmermann's colour series, nos. *c*, *o*, *k*, *g*, *a* and *e* respectively. In addition to these the experimenter had a number of cards specially prepared giving each of the four primary colours in a large range of tints[2] corresponding to the different greys employed. The papers, which measured 1 ft. 2 in. × 11 in., were all mounted on cards. Several duplicates were used so that one card could at any time be substituted for another of the same tint or brightness. The following were among the chief precautions taken to prevent discrimination by extraneous means:

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[1] This series of greys is prepared by R. Nendel of Leipzig. It consists of fifty grey papers ranging from white (no. 1) to black (no. 50) and so graded that there exists between each successive member of the series a difference of brightness which is just discernible by man. The greys used by Samojloff and Pheophilaktowa were selected entirely from this series; it was also the series employed by Yerkes (*The Dancing Mouse*, New York, 1907, 114) for his earlier tests on the brightness vision of dancing mice.

[2] 'Tint' will be used in the present paper to denote degree of saturation. Cf. C. S. Myers's *Introduction to Experimental Psychology*, Cambridge, 1911, 17.

1. The cards during presentation were protected by the shutter-glass. This, it was hoped, would reduce to a minimum such slight surface irregularities as might exist in the texture of the paper, and at the same time diminish the chance of recognition by means of smell.
2. No confusion or difference in facility of reaction was noted on the occasions when the specially prepared cards were substituted for their equivalents from Zimmermann's series, though the surface of the two sets differed perceptibly and probably some slight difference in odour also existed.
3. Any possible difference in the form and size of card was precluded by the constant factor of the shutter-frames which regulated size and shape.
4. Occasionally duplicate cards bearing small marks were interpolated into the series. In no case did any confusion result, the animals appearing indifferent to the presence or absence of such marks. The assumption that discrimination was in no case due to the presence of accidental marks so small as to have escaped the observer's notice seems therefore justified.
5. After each test the cards were withdrawn from the shutters, the positions in which they were replaced being changed irregularly throughout the series[1].

In all, seven animals (the breed, age and sex of which are recorded in table A, page 129) were tested. Of these, three had to be discarded during the general 'training series,' as they seemed entirely unsuited to this particular form of experiment, while a fourth, subject B, proved useless on account of the fixity of the 'position error' (page 182). Certain results were obtained from the three remaining subjects, but the whole series of observations was completed only in the case of one subject, J, the tests in his case extending from December 1909 to December 1910.

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[1] The animals, while in the home-box, appeared in every case completely indifferent to the adjustment of the cards.

IV. DESCRIPTION OF THE METHOD AND OF THE ORDER OF SERIES.

Before the actual test series could be given it was essential that the subjects should become acquainted with the apparatus and mode of procedure. To attain this end they were put through a course of 'general training,' in which the following three stages may be clearly distinguished:—

First Stage. All the shutters were left transparent and the subject was placed in the home-box. The experimenter then opened shutter 1 (S_1, fig. 2)[1], and called to the animal to come through the open shutter into the trial-box, biscuit being offered as a further inducement when calling proved ineffectual. As soon as the dog had entered the trial-box the operator closed the shutter and went round to the home-box. Efforts were then made to attract the dog's attention and, when this was secured, biscuit was placed in the home-box, the experimenter returning as quickly as possible to the former position behind the trial-box. The subject having seen the biscuit placed in position would either regard it intently through one of the shutters, or else run eagerly from shutter to shutter seeking some way of obtaining the attractive morsel. Directly a dog came to a definite halt before any one of the four shutters the experimenter would open it, closing it again as soon as the animal had re-entered the home-box. This process was repeated until the dogs had learnt to regard the shutters as the central point of interest, and would pass readily through them in either direction.

Second Stage. Cards of a medium grey (Nendel's series, no. 20) were placed in shutters 2, 3, 4 and 5 (S_2 S_3 S_4 S_5, fig. 2), thereby rendering them opaque. The mode of procedure adopted during the earlier tests was in all respects similar to that followed in the first stage; but as the subjects became more familiar with the conditions, and increasingly eager to pass through the shutters to obtain the biscuit in the adjoining compartment, certain modifications were introduced.

(*a*) The subjects learnt to pass through shutter 1, directly it was opened, into the trial-box, where they at once proceeded to the shutter-screen for an eager inspection.

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[1] Throughout the whole series of experiments shutter 1 was used by the subjects only as a means of *entrance* from the home-box to the trial-box, the remaining four shutters never being employed for this purpose.

(β) The biscuit was no longer placed in position in the home-box, but instead was given to an animal as soon as it had regained this compartment.

Third Stage. This stage differed from the preceding stage in that only one of the four shutters, 2, 3, 4 and 5, now contained a grey card, the other three remaining transparent. After each test the card was transferred to a different shutter, each of the four shutters containing the card in turn, and the order followed being irregular. As under these new conditions the subjects were only admitted to the home-box through the opaque shutter, they were now compelled to learn to single out the particular shutter in question by means of its distinguishing mark, the card. Thus selection, involving acceptance and rejection, was necessitated, the dogs soon coming to learn that they were presented with something in the nature of a problem requiring solution. It is of interest to note that in the case of all five dogs who reached this stage, a very few tests sufficed to induce a pronounced and definite rejection of the transparent shutters, whereas a much longer period was required before they learnt to enter with confidence the approach to the card-containing shutter, considerable hesitation and wavering being shown at first. Moreover, the habit, so quickly and easily acquired, of not going up to the transparent shutters proved to be one of extraordinary force and tenacity. The sight of a transparent shutter, even when the animals were in an agitated or careless mood, invariably sufficed to inhibit, sometimes with startling abruptness, the tendency to enter its approach. Further, once the animals had learnt to avoid the empty shutters, not a single instance of mistaken entrance was observed throughout the whole course of the experiments. In the case of one subject it was noticed that after three months' holiday the inhibiting effect of the transparent shutters was as well marked as ever.

Colour preference series. This series was given on December 9 and 10, 1909, and January 18, 1910, preparatory to the 'test series proper' in order to discover whether any initial colour preference or brightness preference was indicated in the choices made by the dogs. Four cards, max. blue, max. red, max. yellow and max. green (page 133), were used. In each test all four cards were simultaneously exposed, but the position occupied by each colour was varied irregularly from test to test, care being taken, however, that in the whole series each shutter had been filled an equal number of times with every colour. Seventy tests were given. The respective frequency with which the four colours were selected by subject J was as follows:—red 32, blue 26, yellow 8, green 4.

From these figures it will be evident that a marked predisposition to 'select' red and blue in preference to yellow and green is shown. But it must be observed that the relative frequency with which the two former cards were chosen does not necessarily imply an actual colour preference. Indeed the following facts suggest that the selection shown was based solely on discrimination of brightness difference:—

(*a*) The red and blue used appeared much darker to the human eye than the green and yellow; and the results obtained from subsequent series (page 157) gave reason to suppose that a similar relation held between these two pairs in the case of the various dogs tested.

(*β*) J's reactions were characterised by a definite and immediate rejection of yellow and green together with an indifferent acceptance of blue and red. At this stage the first two colours appeared to exercise an inhibitory influence analogous to that produced by the transparent shutters (page 136). It should be noticed that this subject did not appear to discriminate between the two members contained in either brightness group, but only between the two groups themselves[1].

Unfortunately the attempt made to test the other subjects with similar preference series was rendered useless by the interference of, what will later be described as, the 'position error' (page 182).

Approximate brightness value series, A and B. This series was given, as its name implies, in order to determine, as nearly as possible, the brightness values for the dogs of the standard colours employed. As in the preceding series, Zimmermann's red, blue, yellow and green were used, but in addition eleven cards were selected from Nendel's grey series (nos. 1, 5, 10, 15, 20, 25, 30, 35, 40, 45, 50). The test series proper were preceded by 'training series,' all three series being completed for one colour before the next was presented. In J's case the colours were tested in the order of their degree of attractiveness (blue, red, yellow, green), as estimated by the results of the first series of the 'colour preference tests.' The following procedure was adopted in the 'training series':

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[1] The fact that the brightness difference between the grey (no. 20) used in the 'general training' series and max. red and max. blue was relatively inconsiderable, whereas that between the former and max. yellow and max. green was very much greater, suggests that the rejection of the latter colours in the present preference series may have been due to their brightness novelty. This supposition receives support from the reversal of preference subsequently observed. For, once the subjects had grown accustomed, in the later 'approximate brightness value series,' to accept the brighter colours, the superior attraction these had for them over the darker became clearly evident (page 154). It is of interest to note that the results obtained by Graber indicate that his subjects also preferred the brighter luminosities to the less bright.

(1) The particular colour chosen was presented to the subject simultaneously with the grey most remote from it in brightness[1]. When the animal (by entering the approach which led to the shutter containing the coloured card in at least 70 % of the tests) had given proof of its ability to discriminate between these two, the next grey, differing only slightly less in brightness[2], was substituted for the 'learnt' grey. This process, sometimes lasting over several days, was continued until the dog had given proof of its ability to discriminate the standard colour from all eleven greys in turn.

(2) All the greys, in the same order as before, were presented on the same day for discrimination from the standard colour, each grey being exposed five times in succession. This process, in its turn, was repeated daily until the subject had shown ability, under these conditions also, to discriminate the standard colour from the grey variables, whereupon the 'training series' was considered complete.

Approximate brightness value series A. In this series, as in the second portion of the test series, all the eleven greys were presented daily for discrimination from the standard colour, each grey being given five times in succession[3]. The difference between the two series lay in the order of presentation of the greys, which, instead of being graded as before, was now irregular, *e.g.* 20, 5, 40, 30, 1, 45, 10, 25, 50, 15, 35. Similar tests were given on six successive days. On each of the first three days the greys were presented in a different irregular sequence, exactly the reverse order being followed on the last three days. At the completion of this series the experimenter was in possession of a record of 330 reactions, amounting to 30 tests for each value of grey employed.

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[1] In this, as in most succeeding series, only two cards were presented at a time and always in adjacent shutters.

[2] When the standard colour was of a medium brightness the greys were learnt from each end alternately, *e.g.* 1, 50, 5, 45, 10, 40, etc.

[3] In both the 'approximate brightness value series,' A and B, in the 'repeat brightness value series,' as well as in both forms of the 'grey difference threshold series,' the particular colour or grey used as standard (the subject in all these series always being required to enter the approach to the standard and never that leading to the variable), was each day, previous to the actual test series, presented alone ten times in succession to the dog in order to serve as some sort of clue or reminder as to the particular card to be selected.

On examining the data thus obtained, it was found that in every case there existed some definite region of the greys in which the percentage of right reactions was considerably smaller than elsewhere. Another series—*the approximate brightness value series B*—was therefore given in order to discover how far the percentage in this region could be raised by further practice. In each case the greys employed in these new tests were selected entirely from the region which had yielded the smallest percentage of right reactions. Five different greys, varying only slightly in brightness, were used with each standard colour (table III[1]). Each variable was presented ten times daily with the standard instead of five times as in the previous series. There were no 'training' series. As soon as the tests of series A had been completed for one colour, the same colour was presented under the conditions of series B, and was tested on four consecutive days. In all other respects the procedure in series B resembled that in series A.

The order in which the standard colours were tested in the case of each subject was as follows:—subject J—blue, red, yellow, green; subject T—blue, red; subject R—red, green. The three series of tests (the training series and series A and B) were completed for one colour before the next was presented.

An examination of the data yielded by the 'approximate brightness value tests' in the case of subject J revealed the existence of marked differences in the results obtained for the various colours (table I[1]). It was found that the average percentage of right reactions was very much higher in the case of green and yellow, than in that of blue and red; further, that the first colour tested, blue, had the smallest, the last colour tested, green, the largest, percentage of right cases. This correspondence between the proportion of right cases and the order of testing, together with the fact that several weeks elapsed between the completion of the blue and of the green series, suggested the possibility that such differences as were found to exist were due rather to gradual general improvement effected by increased practice, than to the existence of specific qualitative differences.

In order to test this supposition a new series—*the repeat brightness value series*—was given. In this series all four colours were re-tested. The tests were continued for sixteen days, falling into four groups of four days each. During the first group a different standard colour was tested on each day; in the remaining three groups the same colours were again tested, a different order being followed in each group. Thus for the first time in these experiments the standard colour was changed from day to day. These precautions, it was hoped, would suffice to insure the practice level being approximately the same in the new series for all four colours. For the rest, the procedure adopted resembled, save in one feature, that employed in the 'approximate brightness value tests,' series A. The exceptional feature consisted in the order of presentation of the greys. In the 'repeat brightness value tests,' in every alternate series for each colour the five tests for each value of grey, instead of being given consecutively ('group' method), were separated and distributed over the whole series for the day ('dispersed' method), the different values being given as before in irregular order.

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[1] These tables are contained in the appendix to this paper (pp. 194—198).

On the completion of these tests, the *hue discrimination, series A* and *B*, were given.

Series A. It had been found that subject J had much greater difficulty in discriminating green and yellow from greys 1 and 2[1] than from the darker greys (tables I and III); also, that blue was readily confused with grey 30 (table I), and red with grey 25 (tables I and III). Thus this subject, if choosing on the basis of brightness alone, should have been completely baffled when repeatedly confronted with the following pairs of cards in irregular sequence,

Grey 2 with red (max.),
„ „ „ blue („),
Yellow (max.)} with grey 25 or grey 30, according as the pair
Green („)} containing red, or blue, was next presented[2].

A remote possibility existed, it is true, that the subject's memory for absolute brightness might be of remarkable delicacy, but his behaviour throughout the whole course of the experiments, together with the confusion exhibited in the 'grey difference threshold series,' made against any such assumption. Therefore, under these conditions, differences of brightness being practically negligible, a large proportion of correct reactions would seem to indicate an ability to discriminate to some extent between differences of hue. It should be noted that in this series there was no longer any distinction of standard and variable, the number of coloured cards corresponding to the number of grey. The subject, as before, was required always to select the coloured card; but all four cards being presented on the same day in varied sequence, the colour to be chosen varied from test to test. No preliminary training was given. The one drawback to this series consisted in the fact that the pairs of cards, between which an approximately similar brightness interval had been found to exist, were so limited in number. On this account it was valueless to prolong the series for any length of time, and in order to obviate the necessity of fixed pairs involving constant repetition, a new series was given.

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[1] The difference in brightness between grey 2 and white is extremely small.

[2] The difference in brightness (brightness interval) between the members of each pair was approximately the same in all four cases and well above the subject's brightness difference threshold.

Series B. This series made use of the specially prepared cards, giving each of the primary colours in a wide range of tints, mentioned on page 133. The method was as follows:—Two highly saturated cards of different colour were selected between which there was reason to suppose that only the slightest brightness difference if any, existed for the subjects. The colour chosen for standard was then presented alone ten times in succession; afterwards the two colours were displayed together, their positions being varied irregularly, but each shutter being filled an equal number of times by either colour. The dog was required to select from the pair that colour which, at the commencement of the tests, had been presented alone. As the tests proceeded, other cards of the same hue but differing in tint were substituted for those originally presented. Thus it was possible, as far as the experimenter was concerned, that, during a single series, the two cards in the various pairs successively presented might stand in any one of the following relations to each other:

1. Equal in brightness and in degree of saturation, differing solely in hue. (Several of such pairs were given, the only difference between the various pairs consisting in the degree of saturation or in the position occupied in the brightness scale.)
2. The standard colour might be either slightly, or very obviously, darker than the other.
3. The standard colour might be either slightly, or very obviously, lighter than the other.

These relations were varied quite irregularly. The experimenter was satisfied, by careful observation of the animals' behaviour and a comparison of the results obtained from the other series, that in the majority of cases the various changes resulted, for the dogs also, in a corresponding reversal of brightness relations.

The chief features of this series were (*a*) the wide range of brightness covered by both sets of cards, (*b*) the continual change in the relation of the two members of the various pairs, and (c) the substitution for the standard card of a standard series, in which the only constant was hue. These precautions would seem to exclude the possibility of any systematic discrimination in the absence of at least a rudimentary colour sense. A high percentage of right reactions, under the conditions of the present test series, must therefore be taken to indicate an ability on the part of the subject to discriminate between differences of hue as opposed merely to differences of brightness.

The particular pairs of colours tested were blue and red, blue and yellow, and red and green.

Grey difference threshold series, A and B. This series formed the complement of the 'approximate brightness value series.' It was given in order to discover whether the threshold for the appreciation of brightness difference was as delicate when the animal was required to distinguish between two greys, as when the cards between which it had to discriminate consisted of a colour and a grey. Should the threshold prove to be lower in the latter case, it would suggest that the subject was able to make use of differences of colour as an aid to discrimination when differences of brightness alone proved insufficient.

Series A. The procedure adopted in this series exactly resembled, in all its main details, that employed in the 'approximate brightness value tests,' series A; the only points of difference were the substitution of grey 30 for the standard colour, and of grey 25_n for grey 30 in the variables. 'Preliminary training' series were given as before.

When the region of the difference threshold for grey 30 had been thus roughly determined, a new series was given in order to obtain a more approximate value.

Series B. In this new series six variables (greys 40, 35, 25_o, 25_n, 20 and 15) were used. Each day the tests were grouped into two parts. All six greys were presented, in irregular order and each five times consecutively, in either part; but the order in which the different values of greys were given in the first part was precisely reversed in the second part. The tests were continued for eight days. For the rest, the procedure of this series was similar to that of series B in the 'approximate brightness value' tests.

In order to render the method employed as rigorous as possible, the conditions of the 'grey difference threshold series' were arranged so as to be specially advantageous to the production of a low difference threshold. Among the chief advantages of this series were the following:

(*a*) The tests of the 'grey difference threshold series' were given at a late date in order that the subjects might profit by practice obtained in the previous series; and also in order that (granted they had the ability to discriminate between hue and brightness), the time which had elapsed since the completion of the 'approximate,' and the 'repeat,' 'brightness value' series (when a habit of always rejecting the grey was acquired), might allow of the weakening of this same habit.

(*b*) Since the animals' behaviour in the 'approximate brightness value' series as well as in the 'hue discrimination' series B had seemed to indicate that after a fortnight's rest the gain in discrimination due to increased interest far more than compensates for corresponding loss of practice (pages 162, 175), the subjects, previous to being tested for the 'grey difference threshold,' were allowed a long holiday.

(*c*) Lastly, in order that every chance for improvement might be given, the tests both of series A and B were continued for twice as long (12 days and 8 days respectively) as those of the corresponding series in the 'approximate brightness value tests.'

9. *Colour threshold series.* In view of the fact that in the earlier series certain colours had been found to exercise a unique effect upon J's behaviour (page 167), it was thought that it might prove of interest to determine the animal's threshold for the four primary colours in order to ascertain whether any marked differences existed between them. It was also deemed advisable that at the same time an attempt should be made in like manner to determine the grey threshold, so that the thresholds for colour and for brightness might be compared. Thus the 'colour threshold series' was given.

Grey and the four primary colours were used, these last being very bright and of a low saturation. All five were daily presented for discrimination from Nendel's white, the tests each day falling into two groups. In both groups each colour as well as the grey was tested ten times in succession, but the order in which the variables were presented in the first group was reversed in the second group. As soon as evidence was obtained of the subject's ability to discriminate between any one of the variables and the standard white, another less saturated card of the same hue was substituted for the 'learnt' variable[1]; when this in its turn was also 'learnt,' a similar substitution was again made. This process was continued until the variables had reached such a low degree of saturation that the subject, even after several days, proved unable to distinguish it from the white. The tests extended over twenty-nine days.

It should be noted that in this series the customary conditions were reversed, the subject now being required to reject the standard card (white) and enter instead the approach leading to the variable.

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[1] In the case of the grey the substituted card was always just perceptibly brighter than the one which it replaced, Nendel's greys, nos. 5, 4, 3, 2, being employed.

From this brief description of the various series it will be evident that the method employed throughout the experiments was one which allowed the subject to compare freely in each test the particular cards presented for discrimination. In this respect it may be contrasted with the 'absolute' method used both by Orbeli (page 123) and by Kalischer (page 127). According to the latter method, only successive comparison was possible, as the greys and colours were presented singly.

System of marks. In order that the results obtained from the different tests might be presented in tabular form, a system of marks was adopted. This system, which was based on careful observations of the behaviour of the animals during experimentation, was genetic in character; moreover it assumed the method of comparison, and also that discrimination would usually involve the rejection of the wrong card previous to the acceptance of the right. The particular features in the animals' behaviour selected to serve as units of discrimination are given below, together with the values arbitrarily assigned them. Two slightly different sets of values were used, corresponding to the two methods of marking employed: of these two methods the second ('percentage' method) was a simple derivative of the first ('graded' method).

In general it was found that the dogs, when at a distance from the shutters of from more than 1 ft. to 2½ ft., were unable to discriminate between the different cards presented (page 148). On account of this infirmity they were allowed freely to approach the entrance leading to any shutter, as this apparently constituted one of the necessary preliminaries to careful discrimination. It was not until a subject attempted actually to enter an approach that its behaviour was registered as right or wrong.

The following marks were used:—

I. *Graded Method.*

Marks	Value	Significance
+ +	2	When the subject previous to accepting the right card definitely rejected the wrong; then entered the approach leading to the shutter containing the former, and waited.
+	1	When the animal partially entered the wrong shutter[1], immediately realised its mistake, retreated, sought the right shutter and remained there. This mark might indicate very careful discrimination where the test was difficult, or very careless discrimination where it was easy.

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[1] The expressions 'right,' and 'wrong,' 'shutter' are used to denote respectively the shutters containing the right and the wrong cards.

Marks	Value	Significance
0	2	When the animal, without having glanced at the wrong card, approached the shutter containing the right card, entered and waited. Its value was extremely ambiguous, and except in the 'hue discrimination series' and in the 'colour threshold series' (where the number of tests (100) given each day was too many to allow of any repetition), the 0 cases were neglected.
?	– 1	When the subject entered either the wrong or the right shutter, but hesitated in a marked manner and was unable to settle; or when he was uneasy and refused to accept any shutter.
–	– 2	When the subject having first seen the right shutter and rejected it, accepted the wrong shutter and settled; or when he immediately accepted the wrong shutter and settled.

In regard to the above it should be noted that under mark – two types of cases are included, viz. the cases corresponding to mark 0 as well as those corresponding to mark ++. Since however the 0 cases are generally excluded from the results it follows that the relative proportion of wrong judgments will be slightly increased. The reason of the neglect of the 0 cases was due to the fact that, on the rare occasions of the award of this mark, the behaviour of the subjects generally indicated that they were in a careless and indifferent mood, entering at random the approach to the first opaque shutter discerned, irrespective of what card it chanced to display (page 181).

This graded system of marking is used because the variation in the number of judgments of each grade at different stages in a series is often of considerable interest, frequently serving to indicate the nature of the subjects' discrimination on any particular day. It should be remarked that when the majority of judgments are of any of the following types, certain definite phases of discrimination are denoted; for example,

1. ++ and 0 cases, the level of discrimination indicated is very high.
2. ++ „ ? „ indicates careful endeavour and interest on the part of the subject, discrimination increasing in inverse ratio to the number of ? cases present.
3. + „ ? „ indicates concentration and the desire to solve a 'felt' (?) problem. When the marks consist only of + and ? it may be concluded that the imperfection of discrimination is due to the difficulty of the test, and not to any inattention on the part of the subject.
4. +, ? „ 0 „ Such a combination of marks forms an extraordinarily valuable indication of the *commencement* of discrimination and is very reliable (cf. results of subjects R and J in the 'hue discrimination series' B).
5. 0 „ – „ There is under such circumstances total lack of interest, and of any attempt to discriminate.

II. *Percentage method.* In addition to the above an auxiliary method of marking was employed. This second method was obtained from the 'graded' method by classing together all the positive judgments, ++ and +, as alike right cases, +, and expressing the number thus obtained as a percentage of the total number of judgments.

The absence from the tables of any calculation of the m.v. (mean variation) will occasion surprise; reasons, however, for this apparent neglect are given on pages 176, 177.

Reward and Punishment. It is generally admitted that, under conditions similar to those of the present tests, discrimination is accelerated by employing a combined method of reward and punishment, for, as Yerkes writes, "the conditions upon which the avoidance of a disagreeable factor in the environment depend are far simpler and much more constant than those upon which the seeking of an agreeable factor depend...situations which are potentially disagreeable or injurious repel with a constancy which is remarkable[1]." The reward, in the experiments described in this paper, consisted in making the home-box comfortable, attractive and warm by filling it with straw, and also in giving the subject a piece of biscuit each time that he passed through one of the shutters from the trial-box to the home-box. As a punishment for going to the wrong shutter, a series of slight shocks from an induction coil was given. This procedure however proved totally unsuccessful; for, either the current passed unnoticed, or else produced such intense fear that the animals could not be induced to approach the shutters until several days had elapsed. Moreover no strength of current could be found which produced an effect intermediate between these two[2]; *e.g.* shocks, so slight as to pass apparently unnoticed, were frequently given to subjects T and B in the hope that in time a cumulative effect would be produced which would serve as a warning and gentle deterrent. This hope proved to be a vain one, both subjects, after many days, continuing indifferent to the current. When finally however the shock was barely increased, intense fear was at once exhibited. Indeed so great was this fear that, although subject T had only received very few appreciable shocks and those barely above the apparent threshold, this animal, when two months later replaced in the experiment-box, could not be induced to approach the neighbourhood of the shutters, but instead cowered in a corner and trembled.

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[1] *Op. cit.* 99.

[2] Dr Myers has suggested that this phenomenon may be in part explained on the following assumption.—Suppose the animals to receive a shock which occasions at first only a slight feeling of fear; this feeling in its turn reacts and causes the dogs to perspire; but, with their pads in this condition, the shock (which passes through the paws) would be intensified.

It is interesting to note in this connexion the results obtained in two other experiments where an induction shock was used in order to aid discrimination in dogs. In the one case, G. van T. Hamilton found "that when electricity was finally introduced" the animal's attitude was one of "fear and a wild desire to escape[1]." In the other case the experimenter, Elmer Gates[2], was successful; but the conditions differed in one important essential, viz. only two alternatives, that of receiving the shock or of avoiding it by selection of the right colour, were possible; whereas in Hamilton's experiments and those described in this paper there remained the third alternative of completely avoiding the neighbourhood of the shock, and abandoning all attempt at discrimination.

The use of the induction shock in the case of subject J had a curious result. Some weeks after having received the shocks, he was once more taken down to the laboratory and placed in the box when, though evincing manifest fear, he was soon induced to approach the shutters once more, but always with what appeared to be extreme caution and hesitation. Also, he would never again as before enter the trial-box of his own accord; at first he was pushed in, and later inveigled by means of biscuit. Thus in all succeeding tests with subject J biscuit was given previous to, and not after, selection, and for ten months, though the shock was never repeated, J almost invariably showed restlessness and discomfort when in the trial-box, with evident desire to escape. Further, it was found that the mere dislike of the trial-box constituted an adequate stimulus, even in the absence of a reward (for on certain days biscuit was obstinately refused), to induce this subject, J, to seek the entrance to the home-box.

It has been stated above that biscuit was employed as an inducement and reward; nevertheless the dogs were not starved but received two meals a day. When the experiments took place in the morning, the dog did not receive his first meal until afternoon, and when in the afternoon, the dog was fed at 7 a.m. and exercised during the morning. For the greater part of the day the animals had their liberty and were constantly in and out of the house.

Advantages and disadvantages of the method.

1. *Size of the coloured field.* The important bearing of area on the conditions of colour vision is well known. It was hoped that in the

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[1] "An Experimental Study of an Unusual Type of Reaction in a Dog," *J. of Comp. Neur. and Psychol.* 1907, XVII. 335.

[2] *Op. cit.* 584.

present experiments the size (1 ft. × 10 in.) of the grey and the coloured fields, while not so large as to interfere with the ready comparison of adjacent cards, would yet prove sufficiently great to exclude other objects from the field of vision during discrimination, and to present to the animals a smooth surface of light undisturbed by difference of contour. Despite the size of the cards, however, the dogs, with rare exceptions, showed no sign of ability to discriminate between them without approaching so near that their nose was level with the end of the partitions. Concerning this apparent weakness of sight, similar facts have been described in the case of raccoons[1].

2. *Reflected light.* Watson[2] writes "that the use of coloured papers can never give us a satisfactory test of colour vision in animals." In the face of this assertion by an acknowledged expert a renewed attempt to make use of pigmented papers in order to ascertain the presence or absence of ability to discriminate between light of different wave-lengths requires some justification.

(1) It must at once be admitted that a method of direct illumination is much more delicate than the use of coloured papers, but it may be claimed that the latter, under certain conditions, goes far towards proving that differences of luminosity alone do not suffice to explain the number of correct choices obtained; and that though such a method cannot produce data on which a final pronouncement may be made, still the facts which it brings forward help to clear the way preparatory to the application of the finer methods. (2) Moreover any attempt to determine the limits of sensory discrimination in animals must always have in mind its double bearing on the problem of animal intelligence: for besides the value of any knowledge which may be obtained concerning the ability of an animal to respond to familiar stimuli by more or less artificial modes of reaction, there still remains for examination the more important inquiry into the nature of an animal's behaviour in a normal environment and under normal conditions, together with a re-interpretation of its conduct in the light of the data gained from experiments on sense-perception. For the purposes of this second inquiry it is desirable that the conditions employed in any experimental test shall be as simple as possible, and at the same time shall approximate

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[1] "In fact, they (the raccoons) seemed to be unable to distinguish the food-glass from the others at a distance of more than four or five inches." "Visual Discrimination in Raccoons," Cole and Long, *J. of Comp. Neur. and Psychol.* xix. 678.

[2] "Some Experiments bearing upon Colour Vision in Monkeys," *J. of Comp. Neur. and Psychol.* 1909, xix. 3.

as closely as may be to the surroundings the animal encounters in its daily life. On this score it may be claimed that the use of light reflected from pigmented papers is in some respects superior, and at all events constitutes a most valuable and necessary complement to the more elaborate methods, the very elaborateness of which is chiefly due to an endeavour to eliminate the impurities and complications incidental to everyday experience. (Cf. Watson's attempt to secure monochromatic bands of light, *op. cit.* pages 5—11.)

3. *Simplicity of reaction.* Directed by the same aim of making the conditions of the experiment as simple as possible, the reaction required from the subjects in these tests was merely that of selecting from two large surfaces of light the one to which they had been trained to give a positive reaction, and then to remain standing before it until the shutter was opened, whereupon they were permitted to pass through and so regained the home-box. In addition, the fact that the shutter-screen was placed in such a position as to form a distinct bar to progress ought to have proved favourable in directing the subjects' attention to the important point. The dogs had no system of catches to learn, and the extreme simplicity of the conditions left them free to focus their entire attention on the only feature of the tests which could have been at all unfamiliar, viz. the essential one of discriminating by means of signs of colour or brightness.

According to Thorndike, "the groundwork of animal associations is not the association of *ideas*, but the association of idea or sense-impression with *impulse*[1]." And at one time the writer of the present paper thought that the formation of the desired associations might have been facilitated, had only a more definite motor response been required from the subjects (after the selection of the right card) before the latter were allowed to re-enter the home-box. In the case, however, of certain later tests, *e.g.* the 'approximate brightness value series' for yellow and green, the new associations were formed with such extraordinary rapidity that the necessity for this additional complication became doubtful: moreover, in the course of time most of the dogs introduced a specific motor response of their own by pawing the selected shutter.

4. *The method of comparison.* The behaviour of the subjects indicated that a method which allows of comparison has on the whole certain advantages over an 'absolute' method (page 181).

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[1] *Animal Intelligence*, New York, 1911, 106.

5. *Interest and punishment.* The chief disadvantage was the failure to discover an effective means of punishment; coupled with which was the inadequacy of the reward (biscuit and an escape from the disliked trial-box to the more comfortable conditions of the home-box) as a motive to encourage continued discrimination. Had it only been possible to gain subject J's continued interest, it is highly probable that, in his case, a much larger percentage of right reactions, as well as readier discrimination, would have resulted. The partial or complete failure of the reward was very noticeable in the case of subject B whose attitude in the experiment-box was always one of passivity and indifference. The reward failed under the following conditions with the other subjects also, but to a less extent:

(α) when the test series was unusually prolonged,

(β) when the tests were more than usually difficult,

(γ) when the subjects had been tested regularly, without intermission, for several weeks.

V. RESULTS OF THE VARIOUS SERIES.

The results obtained from the 'colour preference series' having been dealt with already (pages 136, 137) we may proceed at once to an examination of the data yielded by the 'approximate,' and the 'repeat,' 'brightness value' tests.

The 'approximate brightness value' series, A and B, and the 'repeat brightness value' series.

The order for each subject in which the various colours were tested in these series is recorded on page 139; the number of days required for special training in each colour respectively, being given below (table B).

TABLE B.

	Subjects		
Colour	J	T	R
Blue	10	4	
Red	[3] 2	[7]	[9]
Yellow	1		
Green	0		15

The square brackets indicate general preliminary training given previous to the training series proper.

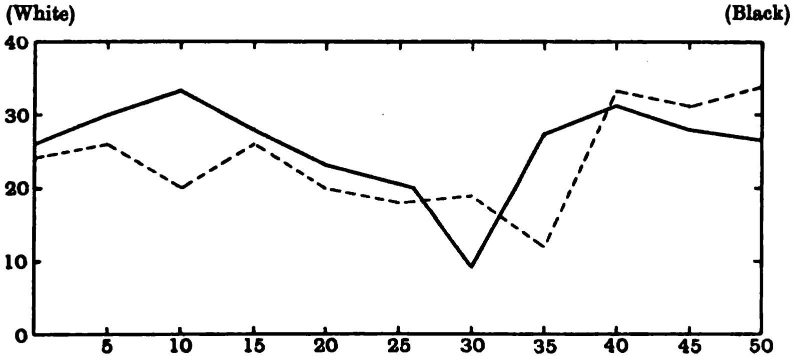

FIG. 3 *a*

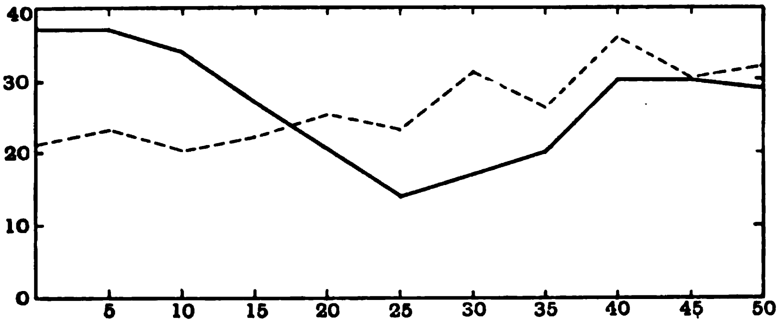

FIG. 3 *b*

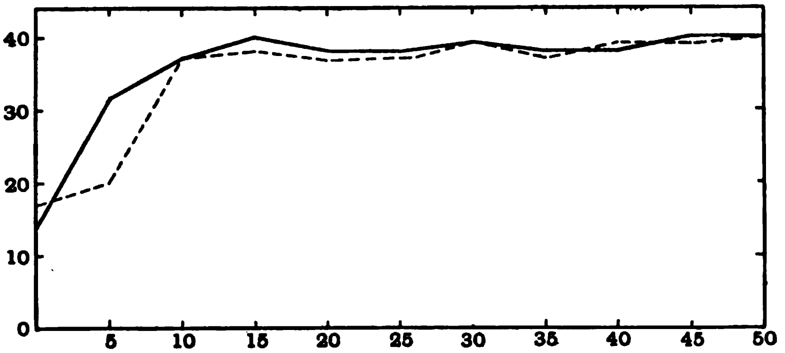

FIG. 3 *c*

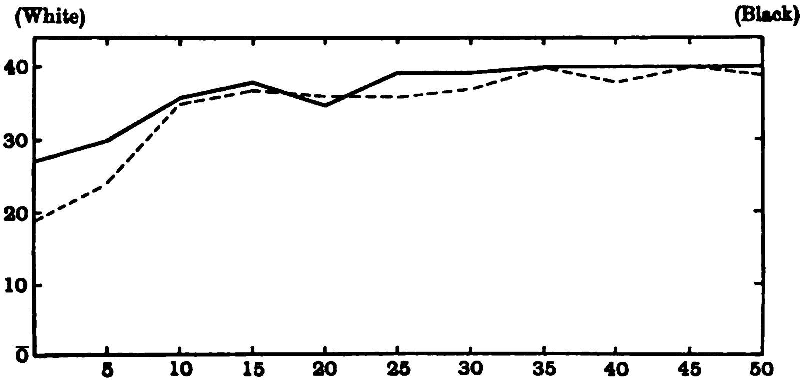

FIG. 3*d*

The above diagrams have been plotted from the total marks (as calculated by the graded method, page 144) obtained, by subject J, for the discrimination of the standard colours from each of the grey variables in the 'repeat brightness value' series ('S_T,' table II), and in the 'approximate brightness value' series A (last four days).

In all four sets of curves (*a*, *b*, *c*, *d*) a continuous line —— is used to represent the curve constructed from the 'approximate brightness value' series A, a broken line - - - - - -, that constructed from the 'repeat' series.

The *a* curves represent the series in which blue was used as standard.
„ *b* „ „ „ red „ „
„ *c* „ „ „ yellow „ „
„ *d* „ „ „ green „ „

The numbers marked off along the abscissae represent the grey variables; those along the ordinates, the degree of discrimination as shown by the graded marks obtained.

A glance at the figures given in the above table B will suffice to show that in the case of subject J the length of time required for training rapidly decreased with each new colour series. This suggests that previous training in the discrimination of one colour from the grey variables subsequently facilitated the discrimination of other colours from these same variables.

Passing now to the results obtained from the test series themselves, several interesting features are to be observed.

An inspection of fig. 3 reveals at once the importance of the part played by brightness in these series.

It will be observed that while all the curves there given agree in exhibiting a marked degree of regularity, at the same time the form of curve varies considerably for the different colours. In the case of yellow and green the curves are relatively low at the white end, ascend steeply and then maintain a high level for the rest of their course. The curves for red and blue, on the contrary, are fairly high at either end but in both cases there is a marked central dip. If we interpret the height of the curve as indicating the degree of discrimination, it is apparent that the region of least discrimination of the standard colours from the greys varied in the four cases. With yellow and green this region was found to be situated at the white end, white and the light greys proving most difficult (judging by the number of right choices) to discriminate from these colours. On the other hand with red and blue it was the medium greys that presented the greatest difficulty while those at either end of the series were fairly readily distinguished in both cases.

That these differences are intrinsic and not accidental is rendered highly probable by the fact that, when different subjects were tested in similar series (table I) as well as when the same subject was re-tested (table II), practically the same peculiarities in the form of curve for the different colours were found to occur. Certain differences however appeared in the 'repeat brightness value' series, and these demand explanation.

The most marked difference was that found to exist between the corresponding curves in the 'approximate brightness value' series A, and the 'repeat brightness value' series, tests (subject J) for the discrimination of standard red from the grey series (fig. 3 *b*). It will be observed that approximately the same level is attained by both curves at the dark end, while in the region of medium greys the curve representing the later ('repeat brightness value') series is considerably higher than that representing the earlier ('approximate brightness value,' A) series. Granted however that the relation between the degree of discrimination and the relative amount of improvement due to practice is asymptotic in character (page 171), such a result is to be anticipated. On the other hand the divergence which occurs at the white end, where the curve representing the later series falls considerably below that representing the earlier, is entirely contrary to expectation. The reason for this fall of the white end of the red-grey discrimination curve ('repeat brightness value' series) was very evident at the time; to explain it the excellent results obtained from the start for the green-grey, and for the yellow-grey, discrimination series in the 'approximate brightness value' tests must be taken into consideration[1]. With two exceptions (greys 1 and 5) these results indicate the presence of a consistently high degree of discrimination; which inference receives additional support from the observation of subject J's behaviour during the two test series. Throughout he exhibited a surprising facility in distinguishing the standard (yellow or green) from the variables (greys), quickly and decidedly rejecting the grey card. The only difficulty occurred in the discrimination of these two standards from white and a light grey (no. 5). (From the first in these tests yellow and green seemed to possess a certain inherent attraction for the subject which was shared by white, an attraction which appeared difficult to resist[1].) In all previous series subject J had been required to reject white and the light greys: in these two series for the first time he was presented with bright standard cards, which were to be selected instead of rejected. And synchronous with this change in the relative brightness of the standard was a marked difference of behaviour, which may best be compared to the change which takes place in an animal's movements when the struggle and difficulty manifested in swimming upstream are replaced, on reversal, by a natural freedom and ease. Further, the yellow and green tests ('approximate brightness value' series A and B) followed immediately upon each other; thus when the subject was tested in the 'repeat brightness value' series he had been accustomed during the past month to select a bright card and to reject cards of medium brightness. It must moreover be borne in mind that during the 'repeat brightness value' tests the standard colour was varied from day to day.

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[1] The greater apparent ease (as shown by the respective graded mark totals—table I) in the discrimination of yellow and of green from the grey series, than in that of blue and red, cannot be explained solely on the ground of increased practice, for precisely the same

The drop in the white end of the 'repeat brightness value' curve is thus attributed to two causes:

(*a*) the apparent attractiveness for the subject of high degrees of luminosity, (β) the previously formed habit of selecting a bright card and rejecting cards of medium brightness.

That there was attraction only and not actual confusion appears from the fact that in the red 'repeat brightness value' series there are

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result occurs in the 'repeat brightness value' series where each colour is tested at the same practice level. It is natural to conclude therefore, that this marked difference is in a great part due to the fact that whereas both the yellow and green employed are for the subjects near one end of the brightness scale, the blue and red on the contrary are situated approximately in the mid-region. The curves obtained for each colour bear out this view (fig. 3 *a*—*d*).

[1] The attraction of yellow and green in the 'approximate,' and 'repeat,' 'brightness value' series is in marked contrast to the results obtained from the 'colour preference' series (page 124). On the other hand it is in accordance with Graber's results who found that his dogs preferred brighter to less bright colours (page 136). Cf. footnote, page 137.

no – cases but only ? cases (table II). Moreover, it was obvious from subject J's behaviour that at different times various colours or luminosities possessed considerable attraction or repulsion for him, an attraction or repulsion so great that the second member of the pair received only the most cursory consideration. Dealing with the particular case of the discrimination of red from white and the light greys we may justifiably suppose that the relative dullness of red in comparison with these latter caused it to be almost unnoticed, the brighter cards attracting the subject's attention first and claiming it most. Notwithstanding the apparent attractiveness of the lighter greys, J never actually accepted one of them, though he always appeared restless and uneasy in their vicinity; there was inhibition, combined with fascination, the subject repeatedly returning to look at a card the perception of which occasioned each time an immediate and apparently 'involuntary' rejection.

That the attractive power of white and the light greys was not apparent in the earlier red series ('approximate brightness value' tests A and B), where, on the whole, cards of a high degree of luminosity were most clearly discriminated against (fig. 3 *b*), is probably to be explained somewhat as follows:—In the first of the 'approximate brightness value' series, blue was used as the standard; and throughout this series white and the light greys obviously proved very attractive to J and difficult to resist, though he appeared to be aware that they were wrong. It may be supposed, therefore, that the repeated struggle in the blue series against the attractiveness of the lighter greys gradually induced in this subject a habit of avoiding the shutters containing bright cards, and that this habit, once formed, was transferred to the succeeding tests, viz. red-grey 'approximate brightness value' series[1].

This fact probably explains also the absence of a marked divergence at the white end of the curves that represent the 'approximate brightness value' series A, and the 'repeat brightness value' series for blue, a divergence which would otherwise be expected since blue and red are of approximately the same brightness (page 157).

A second difference to be noted between the curves representing the 'approximate brightness value' series A, and those representing the

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[1] It is of importance to note that whereas blue throughout appeared to have no attraction for J, red on the other hand appeared in many cases to possess a very obvious attraction. This was shown clearly by the change in his behaviour when red was substituted for blue as standard in the 'approximate brightness value' tests. Red seemed to be definitely 'selected.' The subject employed the 'affirmation,' as opposed to the 'rejection,' method of selection (page 180).

'repeat brightness value' series results from the difference already discussed. It is the transference in the 'repeat' series (red) of the highest end of the curve to the dark greys (table II), whereas in the earlier 'approximate brightness value' series (red) the highest end was found to be furnished by white and the light greys (table I). The point of interest is that this is the only instance in which a transference in the position of the highest end of the curve is observed to take place in the 'repeat' series.

It will thus be seen that in every case there was found to exist a certain region of greys most difficult to discriminate from the standard colour; and that the greater the difference between any grey and this region, the greater the facility with which it was discriminated from the standard colour.

From the foregoing results it cannot be doubted that differences of brightness play a by no means inconsiderable part in the visual discrimination of the subjects, differences of this kind being at first of paramount importance.

It is now necessary to inquire whether in any series there was found a grey which the subject was unable to discriminate from the standard colour.

Let us consider first the results of subject J. In the four 'approximate brightness value' series A, the most difficult case of discrimination appears to be that of standard blue from grey 30, for which a mark of only 60% is obtained (table I). This series was given, however, at a very early period when the subject's training was incomplete, and before improvement, due to practice and an increased familiarity and acquaintance with the conditions of the tests, was attained. Moreover the fact that in the succeeding series B this percentage is raised to 85% (table III) leaves little doubt as to subject J's ability ultimately to discriminate between standard blue and this grey. With red as standard (series A) J's lowest percentage is 70 (red and grey 25), and this percentage remains unaltered in series B. Lastly, the results for both yellow and green (series A and B) leave little doubt as to this subject's ability to discriminate between a green or yellow of the degree of saturation of the standards employed and the grey series.

Thus, in the case of this subject, the results obtained from the 'approximate brightness value' series A and B afford no reason to believe that any one of the greys therein employed is indistinguishable from the four standard colours. This evidence is greatly strengthened by the results obtained from the 'repeat' series (table II). In the latter, it will be remembered, the standard colour was changed from day to day. Moreover the subject's condition had become extremely unfavourable, as he was very tired of, and bored by, the experiments. Therefore the high percentages obtained would demand (on the supposition that dogs are limited to brightness vision) the assumption of an excellent absolute memory for brightness, the contrary of which there is every reason to believe (page 160).

For subject T the evidence is not so convincing. Indeed in series A there is no clear evidence of ability to discriminate between standard blue and grey 25 (46·6̇ %); while even in series B the percentage of right cases is raised only to 67·5 %. The nature of the curve is in both cases noteworthy, the fall to grey 25 being exceptionally steep and abrupt on both sides (table I). Unfortunately this subject became ill, and later was so frightened by the induction shocks (page 146) that, though he was taken down to the laboratory regularly for some weeks, no further results could be obtained.

Likewise subject R in series A (standard green) is apparently unable to distinguish standard green from white (54·1̇6̇ %). On the completion of this series the subject was tested in another series (which extended over four days) in which green was again standard, while max. yellow and greys 1, 2, 3, 4, 5 were used as variables (table III). But even with this limitation of range there is no sign of ability to discriminate standard green from greys 1, 2 and 3. In a 'repeat' series (July 9th—July 12th), however, subject R obtained the percentages 60, 40, 83·3, 100, 80 and 91 for the discrimination of standard green from grey 1, so that it would seem that with practice, and under favourable conditions, discrimination between these two cards is possible though extremely difficult.

Thus, though we cannot determine with any degree of exactitude the point of least discrimination between blue and grey, the results of the two series for subject J, together with those for subject T, go to show that the brightness of blue is roughly somewhere in the region between grey 25 and 35; but for J a careful examination of the data (page 156) shows that under favourable conditions not one of the greys employed was confused with standard blue. The grey most difficult to discriminate from red appears to be grey 25 (series A and B, subject J); whereas for both subjects R and J it is white which is most readily confused with standards green and yellow (tables I and III), thus indicating the extremely high luminosity of these two colours for the animals.

Lastly, a glance at the figures contained in the columns headed 'degree of improvement,' table III, will reveal that the results of series B show, on the whole, a marked improvement upon those of series A. Probably this improvement must be regarded as due chiefly to the limitation of the range of greys in the latter series, rather than as the outcome of increased practice.

Grey difference threshold series, A and B. Though involving a departure from chronological order it is most convenient to examine next the results obtained from the 'grey difference threshold' tests. These tests, which are described on pages 142, 143, really form the complement of the 'approximate brightness value' tests. Their object, it will be remembered, is to discover whether the threshold for the perception of brightness difference is equally delicate when the animal is required to distinguish between two greys, as when he is required to discriminate between a colour and a grey. Should discrimination prove to be more delicate in the latter case it would suggest the presence of a rudimentary colour perception.

It was found that for subject J the brightness of standard blue lay somewhere between greys 27 and 35 (tables I and III), probably in the neighbourhood of grey 30. Thus the two series, the 'approximate brightness value' series A (standard blue), and the 'grey difference threshold' series A (standard grey 30), admit to some extent of comparison. From this point of view it is interesting to examine the data recorded in table I for these almost parallel series, when it will be seen that, despite the highly advantageous conditions under which the 'grey difference threshold' series was given (pages 142, 143) the results obtained for this latter series are nevertheless enormously inferior to those of the earlier 'approximate brightness value' series (standard blue). This inferiority comes out still more strikingly in the tests of series B (table I), where, even with a limited range of greys, subject J gives no evidence of ability to discriminate between the standard (grey 30) and greys 40, 35, 25_0[1], 25_n and 20. This result is in striking contrast to those of the various 'approximate brightness value' tests, series B, where the same subject appears clearly to discriminate between the standard colour and a much narrower range of greys (table III).

Moreover, this absence of improvement in the 'grey difference threshold' tests of series B suggests that the difficulty experienced in discriminating between the various greys is inherent rather than accidental. For, as will be shown later (page 171), there is reason to believe that improvement in discrimination is to some extent asymptotic in character, being more marked in earlier phases where discrimination is difficult than in later phases where a larger number of correct 'choices' are made. But it is evident from a comparison of the results of the 'grey difference threshold' series A, with those of the 'approximate brightness value' series A, that the former represents an early phase[1]. Thus the assumption that in this subject the limits of improvability for brightness discrimination are practically attained seems warranted from the absence of any observed improvement during a relatively long period and under specially favourable conditions.

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[1] Greys 25_0 and 25_n differed considerably in brightness. They were taken from two different packets of Nendel's greys, both alike being numbered 25. The latter, 25_n, was considerably brighter than the former, 25_0, proving, for many observers including the experimenter, almost indistinguishable from grey 18.

Subject R was also submitted to the 'grey difference threshold' training tests, which in her case lasted for thirty-four days. Frequently as many as from 80 to 100 tests were given each day. The standard was grey 25. At first this subject manifested a pronounced preference for the darker greys, which was constant and relative, and interfered with the establishment of discrimination by greatly prolonging the requisite training. Eventually, however, she learnt under favourable conditions to discriminate grey 25 from greys 1, 5, 10, 50, 45, 40 and 35. This discrimination was very transient and disturbed by the slightest modification of conditions. On the whole, her results, though less complete, bear out those of the subject J.

The general conclusion drawn from these series is that the subjects experience greater difficulty in discriminating a medium grey from other greys of medium brightness, than in discriminating standards blue or red from the medium greys most closely approximating to them in brightness.

This conclusion is strikingly corroborated by the behaviour of the subjects during the tests.

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[1] It should be noted that the phrases 'earlier phase' and 'later phase' bear no relation to the amount of practice; *e.g.* the results obtained in the 'approximate brightness value' series A (standard green) were excellent from the first day and would therefore constitute a 'late' phase; whereas, the results obtained in the corresponding 'grey difference threshold' series A, even after a lengthy period of practice, remained extraordinarily poor and would therefore represent an 'early' phase. Briefly, 'early' and 'late' in this connexion refer to the degree of definiteness with which a particular grey or colour is discriminated, and such definiteness may appear perfect from the outset, or remain rudimentary after considerable practice.

(1) The task of discriminating between the various greys appeared arduous; and subject J, though fresh and vigorous when entering the experiment-box, would rapidly tire and become increasingly careless[1].

(2) During the 'grey difference threshold' training series, when the same pair of cards was presented many times in succession until learnt, subject J eventually came to discriminate between the standard grey and each of the variables. When, however, the test series (in which the different variables were presented in turn in irregular order with the standard) was given, this subject evinced the utmost confusion and appeared no longer able to discriminate. He showed no trace either of the formation of an absolute impression or of definite acceptance of the standard card, though previously there had apparently been evidence of the latter in his behaviour in the 'approximate brightness' series (cf. this subject's selection of standard red, footnote to page 155).

(3) Further, preparatory to the test series proper, the experimenter was each day accustomed to present the standard card alone to the subject (footnote, page 138). But it is noteworthy that during all the weeks over which the 'grey difference threshold' tests extended, in marked contrast to his behaviour when one of the colours had been used as standard, subject J invariably manifested extreme disquiet, running up and down the experiment-box, and only approaching with the utmost reluctance the shutter containing the grey card. It is significant that it was only in these tests that J occasionally preferred to enter the approach leading to an empty shutter rather than that leading to the shutter containing grey 30 (contrast page 136).

These features—the confusion in the grey test series, together with the apparent absence of an absolute impression and the reluctance to approach a shutter containing a grey card, which characterize the 'grey difference threshold' series—forcibly suggest that their absence in the 'approximate brightness value' series is due to the subject's ability to make use of a rudimentary colour experience.

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[1] To this carelessness must be attributed the curious case of grey 25_0 in the 'grey difference threshold' series A (table I), where the marks obtained (–16 and –11, graded method) might seem to indicate that there was actual discrimination against the standard. This result is due to the subject, through carelessness or difficulty of discrimination, acquiring a large number of – marks. As was pointed out on page 145, these minus marks embrace two different classes of behaviour, and to the one class (those indicating carelessness) there are no counterbalancing plus marks to denote the instances in which the subject, at once entering the compartment containing the standard card, remained there.

'Hue discrimination' series A and B. It will be remembered that, previous to being subjected to the 'grey difference threshold' series, the animals were tested in the 'hue discrimination' series A and B; these latter tests following the completion of the 'repeat' series. The procedure adopted in the 'hue discrimination' series has already been described (pages 140—142). The results of series A are recorded in table IV. It will be noted, on referring to this table, that with two exceptions (June 1 and 6) the marks obtained, as calculated by both the 'graded' and 'percentage' methods, indicate a surprisingly high degree of discrimination.

There is reason to believe that for subject J the brightness interval was practically the same in the case of all four pairs of cards (pages 140, 141); the conditions of the test involved that sometimes the darker (red or blue), sometimes the brighter (yellow or green), member of a pair should be chosen. The order of presentation was quite irregular. When we further take into account the fact that the subject gave little evidence of ability to form an absolute impression of a given degree of brightness (page 160), we seem forced to the conclusion that the excellent results obtained can only be adequately explained on the assumption that the animal had some rudimentary perception of colour. His behaviour during the tests is of interest in this connexion. The methods of selection adopted by him were as follows:

(1) He carefully regarded the shutters.

(2) He first approached a shutter containing either the grey or the coloured card: in either case there is rejection.

(3) Either (α) he approached a grey after a coloured card: definite rejection, followed by careful comparison of the two cards or immediate acceptance of the coloured card. Or (β) he approached a coloured after a grey card: usually followed by careful comparison.

That is to say, the subject appeared throughout to be reacting to 'not-grey.' There was seldom positive selection of the coloured card; its final acceptance seemed due to the inhibitory effect produced by the empty shutters and by the shutter containing the grey card.

On the other hand, were the subject really guided by a rudimentary colour sense, the absence of increased definiteness and accuracy of discrimination becomes difficult to understand. For, instead of showing greater precision, he became markedly more diffident and hesitant from day to day, and the process of comparison grew longer and more tedious. A similar phenomenon is to be noticed in the 'repeat' series, where the total marks obtained for the different colours show a gradual decrease from the first day to the fourth. The discussion of these cases will be deferred to a later section (pages 173, 174).

Series B. To pass on to an examination of the results obtained from series B (table V):—

The first feature of interest is the lengthy period of preliminary training required by subject J for the discrimination of blue from red.

The earlier portion of the 'training' tests extended from June 8 to June 21, 1910; during which time the subject (despite the advantage of enormously increased practice and the attainment of greater facility in seizing the essential point of the experiment) gave no satisfactory evidence of any ability to discriminate between the two colours in question under the conditions of the test. There is every reason, however, to believe that this negative result was in great part due to 'fag' and to consequent lack of interest; both of which were indicated by the animal's general behaviour at this period, and by the look of extreme lassitude immediately assumed when he was placed in the experiment-box. This view is borne out by the surprising rapidity with which, after a fortnight's holiday, a discrimination habit between blue and red was established. This second period of training occupied only two days (July 14 and 15). As the results are not altogether without interest, the details are given in table V. It should be noted that the subject's behaviour after the rest was in marked contrast to his former indifference: he showed great zest and interest in the experiments, and appeared generally refreshed by the holiday.

Another factor, which might, and probably did, in some part contribute to the indecision shown in the earlier training tests, is that whereas previously the subjects had always been accustomed to give the positive reaction to red—i.e. to select it—they were in the present training tests for the first time required to reject red in favour of blue[1]. Further, it will be remembered that red proved much more attractive to subject J than blue (reference to page 155). Lastly, in his fatigued condition this subject was readily confused by the difficulty of the tests and came to react in a quasi-mechanical manner. From these considerations it can readily be understood that the former habit of responding positively to the more attractive red would be resumed by the subject when in a passive or indifferent condition. And there was in fact found to exist in these earlier tests a very slight actual preference for red; a preference which in part accounted for the delay in establishing a successful discrimination habit between blue and red.

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[1] This subject's memory proved in many instances to be very tenacious (cf. pages 174, 175).

On the completion of the preliminary 'training,' thirty tests, comprising fifteen pairs of different blues and reds, most of which constituted quite novel combinations, were given. It will be seen that in the 'critical' test the percentage of right cases (96·6) is extraordinarily high (table V)[1]. Moreover, this test was immediately followed by another which likewise yielded striking results. In the latter, blues were again used as standards with orange, magenta, yellow, max. red and other reds, none of which had ever been previously presented to the subject, as variables. These variable cards, while differing considerably in luminosity and other respects, all agreed in containing an element of red or yellow. The percentage of right judgments for ten tests (80%) seems remarkably high and strengthens the view that colour played some part in discrimination.

Blue and yellow were the colours next tested. Each blue was presented in turn with the darkest and most saturated yellow, H; and in no case did discrimination present any real difficulty. Even the palest blue, U, which was very bright and only slightly saturated, could be distinguished with ease from yellow H. Yellow, it will be remembered, seemed extremely bright both for this subject and for subject R, yellow max. being more readily confused with white than with any of the greys (tables I and III); and there was reason to believe that even yellow H was brighter for subject J than was blue U[2]. Owing to this considerable difference in brightness it seemed impossible to comply with the conditions of this series, that there should be a constant reversal of brightness relations between the two colours (page 141). Further trials with this pair of colours were therefore abandoned in favour of tests with red and green.

Red-green tests: In order to avoid any complication due to confusion with the previous blue-red tests, in which the negative reaction was given to red, the subject was again required to react negatively to red and positively to green. A remarkably high degree of discrimination was shown in the preliminary tests (six days) (table V). The actual 'critical' test series was continued for four days. Fresh combinations were constantly given and every attempt was made to confuse the subject but, as the table shows, without success.

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[1] Similar 'critical' blue-red tests yielding good results were given on two other occasions, when an even greater diversity of pairs was employed. Unfortunately these particular tests were given in the presence of spectators, and consequently no data were recorded.

[2] For the experimenter in ordinary daylight the brightness of blue U corresponded roughly to that of yellow X.

The positive evidence afforded by the results of these 'hue discrimination' series, together with the combined negative evidence yielded by the 'approximate brightness value' series A and B, and the 'grey difference threshold' series, leaves little doubt that in the case of subject J there is some ability of a rudimentary character to perceive differences in wave-length as opposed to mere differences of luminosity.

Subject R was also tested in a somewhat similar manner with red and green, reacting positively to green and negatively to red. But discrimination between green max. and red max. proved so difficult that a modification was introduced into the method. This modification consisted in presenting three cards (two negative, red, and one positive, green) in each test instead of only two cards (one negative and one positive) as heretofore. This difference served a dual purpose: (*a*) it indicated to which of the two colours the positive reaction should be given; (*b*) it gave the animal a double chance of error, and therefore a double chance of learning through mistakes. The success of this method was shown by a sudden almost immediate increase in the number of right reactions—an improvement maintained over several days with divers pairs of cards. In all, the 'training' tests occupied fourteen days, during which time subject R gave evidence that under these conditions she was able to discriminate between the various greens and reds given in the following table:—

TABLE C. *Subject R.*

Standard (positive)	Variable (negative)	°/₀ age of rt. cases	Standard (positive)	Variable (negative)	°/₀ age of rt. cases
Green F	Red Y	100	Green G_2	Red W	93·75
	,, X	100	,, ,,	,, X	95
	,, X_2	100	,, G	,, W	—
	,, Z	100	,, ,,	,, A—B	90
	,, A	92·30	,, ,,	,, Z	100
	,, A—B	100	,, max.	,, max.	89
	,, W	65	,, B	,, ,,	100
			,, G	,, ,,	100

This table is constructed in a similar manner to table V. The letters attached to the colours refer to the letter-scheme, indicative of tints, there explained.

When, however, the 'dispersed' method of the 'critical' tests, involving a constant change of brightness, was substituted for the 'grouped' method of the 'training' tests, obvious confusion resulted.

To return to the results of the 'training' series. Table C shows that while subject R was able to discriminate with considerable facility the relatively dark green F from all the reds down to red W, a coral pink, she could not distinguish this last red from either green F or the slightly darker green G, though she did succeed in distinguishing it from the very darkest green G_2[1]. These facts, taken in connexion with subject R's inability to distinguish green max. from white under normal conditions, suggest that both the red W and the green G in question appeared to her extremely bright, and that (on the supposition that she possessed a rudimentary colour sense) these cards, though of a relatively high degree of saturation, were yet below her colour threshold. Thus owing to the extreme brightness of green for subject R, this test series affords no direct proof either for or against the animal's ability to perceive differences of hue.

'Colour threshold' series. The object of this series was to determine (i) whether, in the case of discrimination from white, the thresholds of the four standard colours presented any marked differences; (ii) whether the brightness threshold (grey and white) appeared to be of greater, equal or less delicacy than the saturation threshold of the various colours.

Only one dog, subject J, was tested. A description of the procedure adopted has been given on page 143.

Unfortunately the results obtained from the 'colour threshold' series are somewhat indefinite and unsatisfactory. This lack of definiteness and precision may be ascribed in part to the fact that the subject had ceased to be interested in the experiments and no longer devoted his best attention to the task; and in part to the enormous length of time required to determine the actual threshold. For it appeared that the threshold could be lowered to a surprising extent by practice, but that each time the saturation of the variable was diminished an increasing amount of time was required to establish discrimination. Lastly, the present method was unsuited to this particular inquiry, which could be much more satisfactorily and exactly prosecuted by the use of direct illumination.

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[1] For the experimenter in ordinary daylight the brightness of green G_2 corresponded roughly to that of red Y.

The results, such as they are, are recorded in table VI. There is little to be concluded from them, save perhaps that grey and red[1] are more readily discriminated from white than are green, blue and yellow. At all events, the percentage secured in the case of yellow is less than in that of the other colours, though to the observer the degree of saturation of yellow V greatly exceeded that of red T_z, blue V, or green W, and differed from white far more than did greys 3 and 2. Whether this extremely high threshold for yellow is due to some physiological effect of the well-known brightness of yellow in daylight vision, or whether it merely indicates weakness or insensitiveness of the eye to this portion of the spectrum, cannot be decided on the above data. On the other hand, as red is more easily discriminated than the other colours from white, it is possible that red may be much darker for the dog than for man, as Yerkes believes it to be in the case of the dancing mouse[2]. This, however, is rendered improbable by the consideration that in the case of human vision there seem to exist certain broad correlations between red as the darkest portion of the spectrum and green as the point of maximal brightness (as seen, *e.g.*, in the scoterythrous type of colour-blindness and in the dark-adapted eye); and between violet as the darkest portion of the spectrum and yellow as the point of maximal brightness (*e.g.* normal daylight vision and the photerythrous type of colour-blindness). But the 'approximate brightness value' tests indicated that the brightness of standard red for this subject lay somewhere in the neighbourhood of grey 25 (page 157), a grey which, for the experimenter, was very slightly brighter than the red in question. This finding is in accord with the extreme brightness accredited to yellow. Thus both red and yellow fall into the usual scheme of brightness relation. The slight advantage shown for red-white discrimination in the present series must therefore be attributed to either the aforementioned imperfection of these tests, or to a special retinal sensitiveness to the shorter light waves.

A final point of interest disclosed by our data is that the discrimination of greys 2 and 3, which differed only slightly from the standard (white), presented no greater difficulty to subject J than did yellow V, blue U or red T_y (table VI), whereas the reverse held good for the experimenter. In other words a small difference in brightness was more readily perceived by the animal than a *low* degree of colour[1], a fact which affords further evidence that the colour sense, if present, is very weak.

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[1] The degree of saturation of red T_y appeared to the experimenter much lower than that of blue U, green W or yellow V.

[2] *Op. cit.* 175, 177.

VI. SPECIFIC EFFECTS OF THE PRIMARY COLOURS.

From time to time during the investigation there have been indications which suggest that the different colours exert a unique and specific physiological influence upon the subjects, and which in so far point to the presence of a qualitative factor. In support of this contention the following facts may be cited.

(1) The substitution of standard red for standard blue in the 'approximate brightness value' series A was accompanied by a marked change in subject J's manner of selection (footnote to page 155). The animal showed great eagerness and his judgments became much more definite. Briefly, the type of reaction changed from negative to positive[2].

(2) In the earlier 'training' tests for blue-red discrimination ('hue discrimination' series B) it was found that red still proved more attractive than blue, the measure of its attractiveness being shown by the fact that on several occasions it was even selected against position (pages 162, 176).

(3) Whereas in the 'repeat' series there was marked improvement in the total graded marks for each day for blue, the opposite held good for red (table II). This anomaly may in part be explained by the assumption that blue was to subject J a relatively uninteresting colour, difficult to discriminate; and that owing to this early phase[3] of discrimination it admitted of considerable improvement with increased practice (page 171).

(4) Consistent with this explanation is the improvement shown for blue in the 'colour threshold' series, where there is a marked increase in the percentage in the second half of each day's test over that of the first half (table VI). Though in every instance some improvement occurs, that shown for blue is strikingly larger than in the case of the other colours.

[1] It will be remembered that discrimination between a *well-saturated* colour, *e.g.* max. blue, and the greys closely approximating it in brightness proved, on the contrary, more delicate than pure brightness discrimination (page 159).

[2] Kalischer also found that the different colours occasioned characteristic responses. *Vide supra*, page 128.

[3] See footnote to page 159.

(5) The total number of ? and – marks in the 'hue discrimination' series A was 39. Their distribution among the various colours was as follows: blue 19, red 9, yellow 7, green 4 (table IV). Again the great difference existing between blue and the other colours is clearly manifest. Blue, it would seem, is less attractive and has, if the expression be permitted, a less positive character for subject J than that possessed by the other colours.

Brightness vision alone is inadequate to account for the facts just set forth. The constant and well-marked characteristic effects, produced by blue and red respectively, demand for their explanation the sensitiveness of the subject in some manner to differences of hue.

VII. SUMMARY OF RESULTS.

A. *Evidence in support of the view that dogs are only able to perceive differences of brightness.*

1. The result of the 'colour preference' series (pages 136, 137).
2. The nature of the curves obtained for each colour in the 'approximate brightness value' series A. The curves are very regular and show clearly that the greater the difference in brightness between a standard colour and a grey, the more definite and assured is discrimination (pages 150—156).
3. The effect of white, and the change in the form of red curve from the 'approximate brightness value' series A to the 'repeat' series (pages 153—157).
4. The length of time required to establish a successful discrimination habit between blue and red ('hue discrimination' series B, page 162)[1].
5. The fact that whereas change of colour frequently appeared to pass unnoticed (*e.g.* in the 'repeat' series with a daily change of standard, and in the 'hue discrimination' series A with a continual change of standard), a marked change of brightness often caused confusion ('grey difference threshold' series, page 160).
6. The fact that green and yellow were not at all, or only with the greatest difficulty, discriminated from white (tables I and III).
7. The fact that during a prolonged series increased confusion and uncertainty in discrimination were shown from day to day (page 161).

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[1] An alternative explanation was suggested on page 155.

B. *Evidence in support of the view that dogs possess rudimentary colour discrimination.*

1. The decreased time for training required by each new colour in the 'approximate brightness value' series A (page 152). This fact is more readily explained on the supposition that the subject reacts to 'not-grey is right' than that in each case he quickly accommodates to a new standard brightness value[1].

2. The unique physiological effects produced by the different colours even when of approximately the same brightness (pages 167, 168).

3. The evidence afforded by the 'approximate brightness value' series A and B, that on the whole no grey was found that was indistinguishable from the standard colours employed; training being still imperfect (page 157).

4. The difficulty and confusion of the 'grey difference threshold' series as compared with the parallel 'approximate brightness value' series; and the differences in the percentages obtained in the two cases (pages 158, 159).

5. The refusal to accept isolated greys previous to the 'grey difference threshold' series proper (page 160). This phenomenon was never observed in the case of an isolated colour.

6. The results obtained from the 'hue discrimination' series A (pages 161, 162).

7. The results obtained from the 'hue discrimination' series B (pages 162—165).

8. The difference in the approximate thresholds of the four colours and grey as determined by the 'colour threshold' series (page 165).

A comparison of these two sets of evidence shows us that, whereas the facts recorded under A are not incompatible with the assumption that dogs possess some sort of colour vision, the facts recorded under B, on the other hand, are quite irreconcilable with the view that the subjects are limited to brightness vision.

As to the exact nature of such colour vision we unfortunately know practically nothing. One thing at least seems certain, namely that it is of an extremely rudimentary nature. Further, the results obtained from the 'colour threshold' series, as well as the extraordinary difficulty invariably experienced in discriminating a well-saturated yellow or green from white, and the fact that the dogs always required to approach fairly close to the shutters in order to discriminate (page 148), all indicate that the colour sense is very weak, and the threshold probably relatively high. Lastly, the relations of colour and brightness at this stage appear extremely intimate, the former being definitely affected by changes of brightness. (Cf. the effects of brightness on the nature of the curves for the 'approximate brightness value' series, fig. 3.)

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[1] Cf. also the behaviour described on page 161.

VIII. THE EFFECTS OF PRACTICE.

The gradual improvement shown in many of the series will have already been remarked. A re-examination of the data from this point of view gives us the following indications as to its nature.

Qualitative and quantitative improvement. Two phases of practice effects may be discriminated. In the earlier, *qualitative* improvement (*i.e.* an increase in the proportion of + to − judgments, as calculated by the 'percentage' method described on pages 145, 146) is of supreme importance[1]. In the later, qualitative improvement is superseded by *quantitative* improvement (*i.e.* an increase in the proportion of ++ to + judgments or of ? to − judgments, as calculated by the 'graded' method described on pages 144, 145). An examination of the data contained in the columns headed 'analysis of reactions' (subject J—table I), for example, shows that in passing from the blue through the red and yellow to the green series the qualitative improvement as expressed by the average of the average percentages is in each case quite incommensurate with the quantitative improvement as expressed by the total of the sum totals. A similar instance is to be found in the case of the 'grey difference threshold' series B (table I). There the qualitative improvement shown in the tests of the second four days over those of the first four days is 8·3305 °/₀, whereas the quantitative improvement is 72 out of a total of 132. But perhaps the most striking case of all is the discrimination of yellow from grey 1 in the 'approximate brightness value' series B, where the quantitative improvement shown upon series A is 11·5, while the qualitative improvement is only ·15 °/₀ (table III).

The fact that quantitative improvement continues after qualitative improvement has apparently ceased, indicates that in the long run the improvement shown is due not to increased sensibility, but rather to increased accuracy in the general application of methods; possibly also to a strengthening of the attractive influence of the positive card and of the repellent influence of the negative card[1].

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[1] Such qualitative improvement constituted a marked feature of the various 'training' tests.

Transference of improvement. Some trace of a transference of improvement is evidenced by the fact that there occurred with each successive colour series ('approximate brightness value' tests, series A) a slight increase in the total number of marks gained. The exact figures are as follows: standard blue, 387; standard red, 430; standard yellow, 574; standard green, 608 (table I). This result is confirmed by the decreased amount of time required by each succeeding colour series for preliminary training (table B, page 150).

Daily improvement. In the case of two of the series—the 'grey difference threshold' series B and the 'colour threshold' series—it was possible, on account of the large number of tests given each day (120 and 100 respectively), to obtain a new measure of improvement. This new measure consisted in determining the degree of daily improvement, *i.e.* the amount of improvement which occurred during each day. The tests for each day were divided into two similar groups and the percentages obtained for each group were then compared. Table VI shows that under these conditions distinct, though slight, improvement is shown generally in the second group of the daily tests.

Asymptotic character of improvement. It was found that qualitative improvement is usually greatest where the average percentage calculated for the whole of the series is least. In other words the first effect of practice, in increasing the ratio of right over wrong judgments, is asymptotic in character. This is exemplified in the improvement shown for the blue series as contrasted with its absence in the case of the other colours ('approximate brightness value' series A—subject J, table D). Another example occurs in the tests of series B where the qualitative improvement shown over series A for blue and for red is somewhat greater than that shown for yellow and for green (subject J, table III). In table VI, if we except yellow[2], the least improvement is shown for grey, which has also the highest average percentage. On the other hand, the most marked improvement occurs in the case of blue where the average percentage is lowest. Finally it should be noted that in the 'grey difference threshold' series A the improvement shown in the second group of six days over that of the first group ($14{\cdot}1\dot{8}\dot{2}\,^{\circ}/_{\circ}$) is very considerable (table I), and must probably be attributed to the early phase (see footnote to page 159) existing at the commencement of these tests (table I).

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[1] The expression 'positive' card is used to denote a card which the subject is required to select, 'negative' card, one which he is required to reject.

[2] The discrimination of yellow from white seemed to present certain inherent difficulties (page 166).

The relations brought out in table III under the columns headed 'degree of improvement,' between the 'approximate brightness value' series A and the 'repeat' series, indicate that *after an interval of rest* the loss of qualitative improvement occasioned is greatest where the power of discrimination is least[1]—*i.e.* during the earlier stages of discrimination. A glance at the table reveals the discrepancy which exists between the two varieties of improvement, for whereas the loss of qualitative improvement is most marked in the case of blue, the greatest loss of quantitative improvement occurs in the case of green. As judged by the behaviour of this subject during the tests, this difference is to be accounted for by his increased assurance in the case of the green series, resulting in very marked carelessness[2] with complete disregard of the finer differences, particularly in the case of the lighter greys. The discrepancy between the two measures of improvement does not therefore necessarily imply the unreliability of either, but serves rather to bring out the characteristic features of the two methods.

The results of the 'approximate brightness value' series B indicate that there was on the whole decided improvement of both kinds (*i.e.* qualitative and quantitative) when the range of variables was limited (table III). It should be remembered, however, that one of the characteristics of the 'grey difference threshold' tests was the absence of any such improvement in series B (page 158).

Deterioration with practice. It sometimes happened that although the quantitative form of improvement was maintained the qualitative form showed diminution or even a reversal. Such reversals, mainly resulting from carelessness, frequently accompanied a high degree of definiteness of discrimination. They were especially noticeable in series which covered a large range of luminosities of which many presented no difficulty to the subject while others appeared almost indistinguishable from the standard. Instances of this reversal are to be found in the 'approximate brightness value' series A with red, yellow and green as

[1] Factors which undoubtedly contributed to this loss of improvement were (*a*) the daily change of standard colour in the 'repeat' series (page 189) as contrasted with the 'approximate brightness value' series, (*b*) fatigue.

[2] Such carelessness due to a too easy assurance, together with boredom and indifference, interfered to an enormous extent with the symmetry of the results.

standards, where the second groups of three days show a marked loss of qualitative improvement on the first groups, though as judged by the total averages the degree of discrimination is exceedingly high (table D).

TABLE D.

Colour used as standard	Subject	Percentage method		Graded method		Total no. of tests
		Degree of improvement	Total average percentage	Degree of improvement	Total marks	
Blue	T	-2·6 %	$81\cdot\dot{5}1$	-25	305	330
	J	2·6 %	89	31	387	
Red	J	$-1\cdot\dot{6}$ %	90·5	6	430	
Yellow	J	$-1\cdot\dot{3}$ %	$96\cdot\dot{6}\dot{3}$	12	574	
Green	J	- ·6 %	$98\cdot4\dot{5}$	0	608	
	R	5·6 %	$93\cdot\dot{1}\dot{8}$	42	414	

The above table records the amount of improvement, as calculated by both 'graded' and 'percentage' methods (pages 144—146), in the results of the last three days over those of the first three days, for each of the 'approximate brightness value' series A. The 'percentage' improvement consists in the difference between the average percentages, the 'graded,' in that between the sum totals, of the two sets of days. The sign - indicates loss of improvement in the second three days. 'Total average percentage' and 'total marks' give the average percentage and sum total of graded marks for the six days.

Two definite cases occur in which despite increased practice there is actual loss of improvement. The first of these instances is to be found in a comparison of (*a*) the sum totals of the 'graded' marks obtained on the first, second, third and fourth days respectively of the four colour series of the 'repeat brightness value' tests, and (*b*) the 'percentage' values corresponding thereto (table II). From an examination of these results it will be clear that there was a definitely marked, though slight, tendency at work yielding results which indicate deterioration instead of improvement. This case must be connected with another, namely, that of the 'hue discrimination' series A which was referred to on page 161. These two sets of experiments were given during the same phase of the subject's practice. In both sets the standard, instead of being constant as heretofore, was frequently changed, such change occurring either daily as in the 'repeat' series, or else many times during the same day as in the 'hue discrimination' series A. It is not improbable that the absence of manifestation of improvement is attributable to this constant change of standards. But there was not merely absence of improvement but actually deterioration of behaviour[1]. For strangely enough, in both cases as the series progressed, the subject's behaviour became very much more diffident and hesitant and his uncertainty more pronounced. These traits were quite unmistakeable. At first there was very little confusion and remarkable assurance; but gradually the latter gave way to the former. The subject would hesitate for a long time in going from shutter to shutter, and as a rule in the case of subject J hesitation proved fatal. The only suggestion which can be brought forward to account for this curious behaviour is that at first the subject reacted more or less spontaneously on perceiving the cards, but that later an element of reflexion supervened, bringing with it error. This interpretation is based on observation of the behaviour of the animals during the progress of these tests. Indeed, once a discrimination habit was definitely established, the dog's manner forcibly recalled the behaviour of human subjects in reacting to the tachistoscope, particularly as regards the extraordinary definiteness and decision of the immediate judgments[2].

IX. RETENTIVENESS.

Although the methods employed in these experiments at first presented great difficulty to the subjects, the ability to retain the broad experimental methods when once learnt, nevertheless, proved excellent. The following instances may be cited:—

(*a*) After a fortnight's general training at the outset of the experiments subject J remembered the method completely a month later. Also subject R, with longer training, showed excellent memory after ten weeks' holiday, reacting after the rest in a much more definite and decided manner than previously. Similar results were obtained in the case of subject B.

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[1] D. B. Casteel records a somewhat similar phenomenon in his experiments on the discriminative ability of the painted turtle. He writes as follows:—"When the lines were reduced (from a width of 4 mm.) to a width of 2 mm. on both boxes the first five tests resulted in 80 °/₀ of the trials being correct. With a continuance of 2 mm. line tests this percentage was lowered in five tests to an average of 68 °/₀." "The discriminative ability of the painted turtle," *J. of Anim. Behav.* 1911, I. 18.

[2] Cf. "...the number of distinct judgments is very decidedly greater with the momentary than with the prolonged exposures" (tachistoscopic experiments). "The illusion of compared horizontal and vertical lines," G. Dawes Hicks and W. H. R. Rivers, *Brit. J. of Psychol.* II. 255; "...the momentary readings were precise and consistent; whereas with the prolonged observations the subject oscillated between two judgments and the readings were less consistent." "Practice and the Müller-Lyer illusion," E. O. Lewis, *Brit. J. of Psychol.* II. 303.

(*b*) In the 'approximate brightness value' series A, less time was required by subject J for training to standard yellow than to standard red (table B) although, on account of an injury, the subject had not been tested for red during the preceding fortnight. There was apparently no loss of memory in this interval.

(*c*) The improvement noted in the blue-red training tests ('hue discrimination' series B, subject J) after a fortnight's rest suggests retention of the essential features of the method as a basis upon which renewed interest was able to work (page 162). For subject R with the same holiday the 'repeat' (green) series showed an improvement over the preceding 'approximate brightness value' series B, the increased certainty and alertness of response being especially marked (tables II and III).

(*d*) The fact that, once the subjects had definitely learnt not to approach the transparent shutters (page 136), there never subsequently reappeared any trace of a renewed attempt to mistake them for the card-containing shutter, no matter how long the interval of rest[1]. This was found to be true in the case of all four subjects, J, R, T, and B.

These facts afford evidence of a relatively high degree of general retentiveness over considerable periods. They further suggest that the loss due to waning retentiveness during an average period of rest is relatively inconsiderable, being more than compensated for by renewed interest and increased attention; cf. (*c*).

X. DESCRIPTION OF BEHAVIOUR AND GROWTH OF DISCRIMINATION.

Preparatory to describing the behaviour of the subjects, it may be advisable to say a few words with regard to the nature and conditions of the experiment.

Any attempt to determine the delicacy of sensory discrimination in animals must necessarily labour under great embarrassment, in that we are obliged to employ methods which involve as an essential condition either the formation of an association or the perception of a relation and, in so far, fall short of constituting a pure test of sensory discrimination. To insure that indications of failure to discriminate, or of an apparently low degree of sensibility, are not primarily due to the unsuitability of the methods employed as a means of investigation, the most rigid and penetrating analysis of these means is indispensable. The requisite features of a method employed in this connexion are:—

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[1] The apparent exception noted on page 160 does not really contradict this statement, as the empty shutters were never *mistaken* for the right shutter and were only entered with the utmost reluctance.

1. That it shall be of such a nature and sufficiently simple as to be easily understood by the subject, readily tending to become subordinate, and so leaving his attention free to be centred on the focus of the problem.

2. That the method shall embody conditions which will insure, as constantly as possible, the subject's continued interest and attention.

3. That the method shall not conflict with any well-established instinct or preference.

In order that the data afforded by any experiment designed to test sensory discrimination shall be of value, the above important conditions must be constantly borne in mind throughout their interpretation. A case in point is that of the earlier blue-red 'training' series ('hue discrimination' series B, subject J, page 162). In this series, judging from the proportion of right to wrong cases, there would seem to be complete absence of discrimination. But examination of the subject's behaviour throws considerable doubt on such an interpretation. Throughout the period of training, red clearly proved attractive to the dog, and in the earlier tests was definitely 'preferred'; at a later stage, however, the animal appeared to struggle against the impulse to approach the red card, though when fatigued he would at once succumb to its fascination. Thus the records came to show an almost equal number of red and blue entries, the gradually formed habit of selecting blue being counterbalanced by the superior attractiveness of red. This brief analysis indicates that, far from there being absence of ability to discriminate between blue max. and red max., there was on the contrary every reason to suppose the presence of very definite discrimination. The fault lay with the conditions of the test which in this instance required that the animal should react against a natural preference.

Closely bound up with considerations of method is the nature of the mean variable error (m.v.). In the present tests, as is shown by an examination of the accompanying tables, the limits of individual deviation from the mean were in many instances enormously wide; much wider, indeed, than those normally found to obtain for human subjects in similar tests. Such a difference, far from being contrary to expectation, however, is just what, from an analysis of the facts involved, we should be led to anticipate. Its explanation is to be found in the relatively small extent to which, in the case of man, conditions of method together with complexities of perception interfere with observations on sensory discrimination, as compared with the case of the higher animals. The difference lies in that man is possessed of a finer focus of attention, a greater power of evaluation, of inhibiting irrelevant details and of concentrating attention on the strictly important features of the situation. Thus, the artificial means of attaining a condition approaching a purely sensory discrimination is in his case very delicate and fine. The higher animals, on the other hand, have not this power to the same extent. Their focus of attention is less delicately adjustable, and the present as opposed to the ideal (past or future) dominates consciousness to a greater extent. There does not exist in their case the same possibility of inhibiting interfering factors. It follows that the particular features which from time to time attract attention are only with considerable difficulty relegated to the background of consciousness.

This difference renders sensory tests on man and the higher animals strictly incomparable as regards complexity. Indeed, in view of their relative difficulty, sensory discrimination tests on the higher vertebrates might aptly be compared to complicated intellectual tests on man in which the subject, unable to grasp completely the point at issue, has to rely on a series of sporadic gleams[1]. The results of certain experiments conducted by Vitus Graber are not without interest in this connexion. By means of his 'preference' method Graber endeavoured to test the brightness and colour reactions of more than fifty varieties of animals including worms, insects, birds, reptiles and mammals[2]. He found that, whereas among the lower animals stimulation rarely failed to occasion a direct response, the number of failures which occurred among the higher animals was very considerable. That is to say, reaction to sense stimuli becomes more irregular and the mean variation consequently larger, as the direct sense response is gradually superseded by the increasing complexities of perception. Further, Graber states that the individual differences found to exist among dogs were notably greater than among the other species of animals employed.

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[1] It is well known that in observations on sensory discrimination in peoples of lower culture the mean variation obtained is frequently very small, once the conditions of the test are understood. This is to be explained by the fact that the power of apprehension and attention of primitive man is, in *simple* cases of this kind, not greatly inferior to that of civilised man and vastly superior to that of the higher vertebrates. Presumably also, more primitive man is possessed of a certain advantage in that to him a problem of this nature would present a greater simplicity, possibly a greater interest, than to more civilised man. The primitive subject is thus less liable to distraction, and consequently, in his case, the conditions of the test acquire a greater stability and uniformity.

[2] *Grundlinien zur Erforschung des Helligkeits- und Farbensinnes der Thiere*, Prag, 1884.

The gradual growth of discrimination presented many features of interest. The first to claim attention, and possibly the most important, was the critical period, always well-defined, during which the subjects came to learn that they were being presented with a problem requiring solution, viz. that some shutters would open while others would not and that the shutter which opened was not always the same. The significant question for them was to find in what respect the shutter that would open differed from the other shutters. So long as only one card was used, this difference was in most cases mastered quickly enough, the animals readily learning to reject the transparent shutters. In marked contrast, however, was the behaviour when two or more cards were used, *e.g.* red max. with black or white. It is true that the subjects confined their attention to the shutters containing cards, but in every case it was long before they came to discern the nature of the important difference. This fact is strikingly brought out by the following observations. The dogs, at an early period of the general 'training,' all developed very marked tendencies to fall into 'position' errors. Test after test they would seek the same shutter and wait before it, regardless of its content. In order to discover the strength of visual discrimination in relation to this habit, the following mode of procedure was devised. The subjects were trained by the usual method to select red and to reject black and white; but as soon as discrimination was established this method was modified in an important manner. Instead of occupying different shutters from test to test the red card would be placed in the same shutter for seven successive trials; on the eighth trial however, black or white would be substituted for red in this shutter, the latter colour being transferred to one of the neighbouring shutters. This procedure was frequently repeated. Nevertheless, despite the striking change in brightness when black or white was substituted for red (table I), not one of the five subjects tested evinced the slightest uneasiness or least trace of perception of the difference until after a very lengthy period of training, notwithstanding the fact that they gazed fixedly at the shutters and presented every appearance of attending.

Although the subjects experienced so much difficulty in learning to single out brightness difference or colour difference as the central problem before them, once this relation was thoroughly grasped, they would go freely up and down the trial-box carefully inspecting the shutters until they chanced on one containing a card they could accept[1].

Recognition, the next step in advance was indicated in various characteristic ways:

1. Hurried and determined movement away from a particular card frequently accompanied by a shake of the head and a contemptuous sniff. (Compare the effect of blue light on Kalischer's subjects described above, page 128.)

2. The length of time that the subject would wait in an alert and expectant attitude before a particular card. This constituted a specially valuable index in the case of subject R (page 180).

3. By various motor responses. The dogs frequently pawed the selected shutter, or stamped, showing signs of great impatience; subject R in the earlier phases would even bark. When she 'recognised' a card, this subject would evince every sign of excitement and intense motor agitation; her whole body trembled, and up to two or three minutes the period of waiting only served to increase the excitement[2].

4. Settling. The behaviour of the subjects, indicating at first disquiet and uncertainty, would suddenly, upon their perceiving a certain card, undergo a complete change. All trace of interrogation at once disappeared, and the animal taking up its position in front of the said card would wait for a considerable time in what appeared calm certitude. This type of behaviour was very characteristic and well marked.

Recognition as shown by the first sign may be described as 'negative,' and by the other three signs as 'positive.' Taken together with other general indications these signs afforded an excellent mode of discovering how far at any particular time the subject was actually discriminating, how far reacting in a merely *quasi*-mechanical manner.

The growth of discrimination in subject R is worth special mention. In her case the 'position error' proved harder to eliminate than in that of the other subjects[1]. Owing to the deeply rooted 'position' sense this animal exhibited a very clear type of what may be termed 'tied' discrimination. For many weeks she would, when admitted to the trial-box, approach always the same shutter, waiting before it irrespective of the card displayed. (When the shutter in question happened to be empty, the nearest card-containing shutter to the left was selected.) Her behaviour in other respects, however, was totally different according as this particular shutter chanced to contain the 'positive,' or the 'negative[2],' card. In the latter event she evinced great excitement and interest; in the former, mere passivity and indifference. The chief point of interest here is that, despite the undoubted presence of discrimination and recognition, the impulse should be so completely lacking to leave the wrong card and go in search of the right. Eventually this subject did reach a stage at which the tendency to enter the accustomed shutter was to some extent inhibited when the wrong card was displayed, the animal in this event passing on to examine the other shutters. But any change in the series or standard card caused reversion to the former type of behaviour. It was found that this second stage, which we may term the stage of 'freed' discrimination, was more quickly attained if the experimenter, on the occasions when the wrong card was displayed, called "No" as the subject neared the approach. Frequent use of this device, however, produced confusion and hesitancy resulting finally in the animal refusing to approach any shutter.

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[1] Subject R proved a partial exception to this statement; an account of her behaviour will be found on page 180.

[2] Orbeli noted in his dogs a somewhat similar phenomenon, the 'réaction motrice.' He writes:—"Dès qu'apparaissait sur l'écran la figure éclairée, le chien se mettait à tendre vers elle, à frétiller de la queue, et parfois même exécutait des mouvements de préhension avec la gueule et la patte comme s'il tendait à enlever la figure de l'écran....La réaction motrice était, dans le premier temps, la même, quelle que fût la figure apparue sur l'écran. Mais à partir du moment où nous nous mîmes à annihiler l'action du carré, il s'élabora, à côté de la différence dans l'effet sécrétoire, une différence dans les phénomènes moteurs" ("Réflexes conditionnels du côté de l'œil chez le chien," *Arch. d. scs. biol.* 1909, XIV. 95).

Reaction types. Two contrasted types of reaction were clearly manifested in the behaviour of the animals; they may be termed respectively the 'affirmative' and the 'negative' types. According to the former—the affirmative type—the subject, while appearing indifferent or passive in relation to the second card, seemed to be definitely attracted by the accepted card. The attitude indicated was one of positive acceptance, which might be expressed by the judgment 'this is right.' The reactions of subject R were predominantly of this type; also the reactions of subject J towards a red standard (footnote to page 155). According to the second, or negative, type it was the accepted card which proved unattractive, this card being reluctantly accepted by the subject only after definite and decisive rejection of the second member of the pair. The attitude in this case appeared one of passive acceptance, being best expressed by the judgment, 'this is not wrong.' The majority of the reactions both of subjects J and T clearly belonged to this type.

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[1] It is of interest to note that the data obtained by Yerkes in his experiments on the dancing mouse rather point to the view that the position sense is more fundamental in the case of the female dancer than in that of the male (*op. cit.* pages 276, 277). The present experiments, so far as they go, suggest a similar sex distinction, though the possibility remains that the one female tested, subject B, may not have been, in this respect, a typical individual.

[2] See footnote to page 171 (1).

Method of Comparison. It will be evident that whereas according to the former type there might be definite acceptance of the first card encountered, according to the latter type, on the other hand, definite acceptance could not occur until after both cards had been seen by the subject. This latter condition, the observation prior to selection of both cards, is of importance in that it appeared to facilitate decision and discrimination. For, where with one card alone there was hesitation and uncertainty, the observation of two cards and of the difference between them served to a large extent to dispel such uncertainty and to render the subject definite. In fact, subject T and subject J (during the earlier months especially) constantly employed a method of *comparison*; such a method, however, was only used spasmodically by subject R and in her case did not appear until a much later date.

The mode of procedure adopted in the method of comparison was as follows. After careful examination of those shutters which contained cards, the subject would either walk to and fro between their entries regarding the two cards in turn, or else taking up his stand midway between them would repeatedly glance from one to the other with an air of grave preoccupation (? comparing), until such time as a decision was finally reached[1]. In the last stage, having made his decision, the subject would with great determination enter the approach leading to the selected card and proceed to wait before the latter with an air of assurance and expectancy. It should be noted that this method of comparison was only employed by the subjects when in an interested and careful mood, its use always yielding a high number of correct reactions. Further, the establishment of what may be termed 'absolute' discrimination was almost invariably preceded by this form of 'comparative' discrimination; indeed, the latter appeared to constitute an important pre-condition of the former. It was on this account that the experimenter was inevitably led to the view that Orbeli's failure to elicit any proof of colour discrimination in his dogs must be attributed in part to his use of the 'absolute' method (page 123)[1]; since such a method would require presumably much longer to afford evidence of discrimination than would a method admitting more directly of comparison.

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[1] Colvin and Burford write of one of their subjects:—"He gave the appearance of actually deliberating or 'making up his mind,'...though no elaborate process should be attributed to his consciousness at this time..." (*op. cit.* page 38).

Position errors. As the method of comparison forms the greatest aid to discrimination, so what has already been referred to (page 137) as the 'position error' forms the chief hindrance. The position error assumed two forms: (*a*) The 'absolute' position error, when on admission to the trial-box the subject each time approached the same shutter and waited before it. Except on the occasions when discrimination was of a particularly easy character or the shutter was occupied by a card possessing marked repulsive force, this habitual impulse to enter the accustomed shutter at once, without examination or consideration, could only be inhibited when the subject's mood was marked by a very high degree of interest and attention. (*b*) The 'relative' position error is methodical in character. It consisted in a tendency shown by the animals, in certain instances, to enter the first card-containing shutter encountered. This tendency, moreover, assumed a stereotyped form. It will be remembered that the subjects were always admitted to the trial-box through the same shutter (S_1), which was situated at the extreme left of the shutter-screen (footnote to page 135). When under the influence of this form of position error, the animals, upon admission, would proceed at the foot of the screen towards the right regarding each shutter in turn; and, as soon as the first shutter containing a card was perceived they would, without investigation of the remaining shutters and irrespective of the card displayed, enter its approach and settle. As compared with the absolute position error this relative form was of infrequent occurrence. Though some trace of it was found in all three subjects, J, T and R, it was only in the case of the last-named subject, R, that it constituted a serious factor.

No trace was found of the third possible form of position error noted by both Waugh[2] and Yerkes[3] according to which the subject tends to select the shutter which had previously proved successful.

[1] In this connexion it is interesting to note that Washburn and Bentley in their experiments on colour discrimination in the creek-chub found that whereas this fish gave no evidence of ability to discriminate between colours which were successively presented, it proved able on the other hand to discriminate between the same colours when simultaneously presented. "The establishment of an association involving colour-discrimination in the Creek-Chub, Semotilus atromaculatus," *J. of Comp. Neur. and Psychol.* 1906, XVI. 115-119.

[2] "The rôle of vision in the mental life of the mouse," *J. of Comp. Neur. and Psychol.* 1910, XX. 593.

[3] *The Dancing Mouse*, New York, 1907, 106.

So deeply rooted, however, were the other two forms that much time was spent in an endeavour to eliminate them; and even after the apparent accomplishment of this end, the slightest change in the mood of the subject or in the conditions of the test would suffice to bring about their recurrence. Moreover, it was noted that, with rare exceptions, even a perfect discrimination habit would be upset if the positive card chanced to be placed in a shutter not recently used. In such an event it generally happened that, though evincing the greatest distress and hesitation, the subject would end with a reluctant acceptance of the card recognised as wrong rather than search for the right card in an unaccustomed position. Further, it was found that even after a year's training definite effort was often required to resist the impulse to obey the positional judgment in opposition to the dictates of sense-discrimination.

Self-control. In the development of discrimination a very definite and interesting characteristic was the appearance of what, for lack of a better term, may for the time be named 'self-control.' The following is an instance of the mode of behaviour to which this term was applied. Many cases occurred in which a subject eagerly making for a definite shutter came to a sudden halt at the threshold. From this point of vantage the animal, obviously desirous of entering the approach, would instead remain for some seconds intently regarding the card displayed while, it seemed to the experimenter, a hard conflict was proceeding; at times furtive movements would even be made as though to enter, but of these the subject invariably repented, always retreating before the shutter was reached. Eventually the shutter in question was as a rule decisively rejected, the animal then passing on, somewhat reluctantly, to an investigation of the other shutters. This mode of behaviour was only observed to occur at the level of 'freed' discrimination (page 180), and then chiefly as the outcome of attempts to resist either an insidious position error, or a very attractive negative, *e.g.* white or red. The important point is that both cases seem to imply some species of ideal control.

Inhibition. Another form of behaviour, which may be termed 'inhibition,' must be sharply distinguished from the above. The chief feature characteristic of this phenomenon is that the control instead of proceeding from the subject seems to be forced on him by external conditions, some definite conation being rudely and abruptly interrupted. As, for example, when a subject on being confronted by the emptiness of some shutter which he had eagerly approached, or by the repellent nature of the card contained, is thereby brusquely drawn up at its threshold, the animal's attitude being invariably indicative of surprise. On these occasions no hesitancy, no inner conflict was manifest; instead, always and very suddenly, some impulse was abruptly thwarted and the thwarting invariably proved effective, the subject in many cases leaving the neighbourhood of the shutter not to return during the test[1].

It will by now be abundantly evident that the formation of a definite discrimination habit by no means insured constant correctness thenceforward in discriminating between similar cards. Indeed, the exact contrary was found to hold true, habits of colour and brightness discrimination proving highly unstable and easily disturbed. Brief mention may be made of the more important conditions which involved interference with discrimination.

Conditions serving to mask discrimination:

(*a*) Lack of interest, which might be due to (i) general mood previous to entering the laboratory, (ii) the extreme difficulty of discrimination in the case of certain series, *e.g.* the 'colour threshold' series, or with particular pairs of cards, *e.g.* yellow and white, (iii) fatigue and boredom, (iv) confusion resulting from a continual and rapid change of the standard or variable where discrimination was still somewhat imperfect.

(*b*) Novelty. In some instances novelty proved attractive, the novel card being selected in preference to the more familiar positive card[2]. On the other hand, in the 'grey difference threshold' series, where in subject R's case the variable was used as the positive card, this animal repeatedly refused to accept the required card, apparently on account of its novelty.

(*c*) Preference. In certain instances marked preference was shown for the negative card (page 162). Such preference is probably to be attributed to a unique physiological effect.

(*d*) Position errors. Judgment in relation to position was always present and, unless the subject was very attentive and alert, inhibited the other forms of discrimination.

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[1] The diverse effects described by Kalischer as produced on his subjects by the action of blue and of yellow light respectively (page 128 *supra*) would appear to present typical instances of 'self-control' (yellow) and 'inhibition' (blue).

[2] Dr Myers found that novelty also exercised some influence in the case of the colour discrimination of an infant. "In the first two experiments the blue brick was obviously preferred on the score of novelty whereas subsequently the blue and white bricks were picked up almost equally often." "Some observations on the development of the colour sense," *Brit. J. of Psychol.* II. 356.

(*e*) Elimination. After the decisive rejection of one card of a pair the remaining card was frequently accepted without any examination in a purely automatic manner.

(*f*) Large range of variables. When the range of variables deviated very widely from the standard there was noticeable a distinct tendency to carelessness in the case of those variables which differed least from the standard.

On the other hand, the chief *aids to discrimination* noted were:

(*a*) Interest, arising from (i) freshness and general mood, (ii) curious and inquisitive disposition, (iii) novelty, (iv) stimulation: (*i*) rapidity with which the reactions followed one another[1], (*ii*) tickling, rubbing etc.[2], (*iii*) tantalising with biscuit, (*iv*) the presence of another dog in the room which was spoken to and fed when the subject sulked. This ruse frequently succeeded in arousing the latter to further efforts, in which, moreover, considerable care was shown.

(*b*) The method of comparison including (i) 'cumulative' effect. *E.g.* in cases where the positive card was only slightly attractive and the negative card more or less indifferent renewed comparison often tended to induce the subject to accept the former. Indefiniteness was gradually replaced by increased definiteness as the result of each attempt at comparison.

In a second well-marked form of this effect the subject, standing in front of some shutter, would closely and continuously regard the card displayed. During this process, his attitude manifested growing definiteness which finally culminated in a definite rejection or acceptance of the said card. The animal's manner on such occasions was markedly different from the cases where, owing to the shutter remaining unopened (the card being wrong), a card would be rejected after lengthy waiting.

(*c*) Reinforcing effect. In cases where the positive card was indifferent, the negative card repulsive, the subject, after repeated comparison, tended to select the former despite its indifferent or even slightly distasteful character.

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[1] Watson notes a somewhat similar result in the case of his monkeys. "At first, the stimulations were given at intervals of three minutes, but the animals became so eager and restless during the long wait, that the tests were finally given as rapidly as the apparatus could be arranged." "Some experiments bearing upon colour vision in monkeys," *J. of Comp. Neur. and Psychol.* 1909, XIX. 16.

[2] Cf. McDougall's statement, "It would seem in fact, that, so far from sensory stimuli being detrimental to activity of the higher levels of the brain, all peripheral stimuli promote mental activity so long as they are not powerful enough to attract attention to themselves." "The physiological factors of the attention-process," *Mind*, 1903, N. S. XII. 298.

(*d*) Limitation of the range of variables. It was found that limitation of the range of variables (entailing lessened contrast and a consequent increased importance of small differences) was generally followed by a marked improvement in discrimination.

(*e*) Limitation of position. If one of three shutters, either adjacent pair of which had been used alternatively, was omitted, imperfect discrimination was frequently transformed almost at once into perfect discrimination.

Habit formation and relative plasticity. A discussion of the gradual development of discrimination would not be complete without some account of the nature and rapidity of habit formation under the particular conditions involved by the tests. Among the chief facts of interest observed in this connexion are the following:

1. Whereas the formation of a discrimination habit opposed to a natural preference could only be acquired with the greatest difficulty, proving, even when acquired, highly unstable and readily reverting to the natural condition; the formation of a discrimination habit, starting from initially indifferent material, constituted, on the other hand, a comparatively easy affair, improvement being relatively steady and a high degree of excellence attainable. Such habit, however, despite its comparative steadiness was very easily unlearnt.

2. Definite discrimination habits were more easily formed where the conditions involved by the series were all averagely difficult than in easier series in which greater relative differences occurred.

3. The evidence obtained points on the whole to the assumption that a discrimination habit is more readily formed when the positive card is used as the standard and the negative is used as the variable, than when these conditions are reversed. For under the latter conditions the unfamiliarity of the positive card, while invariably engendering confusion, frequently also led to its rejection.

4. It might naturally be supposed that the employment of the 'grouped' method, where each variable was presented several times consecutively instead of being dispersed throughout the whole series (page 140), would yield results superior to those obtained from the employment of the 'dispersed' method. For all instances where training was fairly imperfect this expectation was found to be justified. But in certain series, after the subjects had acquired an average facility of discrimination (*e.g.* 'approximate brightness value' series, subject R, and the 'repeat' series, subject J) no marked difference was found to result between the employment of the two methods (table II).

As the object of the 'approximate brightness value' series consisted, not in discovering whether discrimination could be shown under complicating conditions, but whether discrimination was possible at all even under favourable conditions, the 'group' method was employed throughout the series with the sole exception of the green series in the case of subject R.

XI. THE RÔLE OF KINAESTHETIC SENSATIONS AND THE POSITION SENSE.

Mention has already been made of the position errors into which the subjects constantly fell, but the importance of the part played by position is so great that it deserves still further notice. Certain facts are suggestive.

(*a*) From the first there was in every case definite trace of a strongly developed positional memory, and this so dominant as for the most part to render useless any attempt to obtain a preference series.

(*b*) The surprising tenacity of the positional reaction habit and the extreme difficulty with which such habit was overcome despite the adoption of many subsidiary devices directed towards this end. Indeed, in the case of the two subjects, R and B, complete success was never attained.

(*c*) The readiness with which the animals, on the slightest change in the subjective or the objective conditions, would revert from a well-established habit of colour discrimination or brightness discrimination to the positional (either absolute or relative) form of reaction[1]. Even under favourable conditions reactions opposed to the latent positional tendency generally seemed to demand a certain degree of effort and resistance.

Taking into consideration the force of this positional habit of reaction even in opposition to the conditions imposed, its presence in all the subjects, and the instability of other forms of habit in comparison to its permanent character, one is inevitably led to the conclusion that sense and memory of position are of far greater importance and significance to the dog than sensations received from light stimuli, just as the variations in the intensity of illumination are of more importance than differences of hue. We are then dealing here with three strata:

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[1] It should be noted that the subjects were *always* proof against the position error in the case of a transparent shutter (page 136), though the perception of the blankness of a shutter frequently occasioned a change from the absolute to the relative form of the error (page 182).

(1) first, and most fundamental, the sense of position;

(2) the ability to discriminate differences of brightness, an ability which only manifests itself when the position sense is inhibited, and which is relatively unstable, tending on any change to revert to mere positional discrimination;

(3) a rudimentary colour discrimination, highly unstable, and which only comes into play after the subject has learnt to neglect (1) and (2).

Anatomical evidence in support of the relation of (3) and (2) is afforded by the experiments of Kalischer who extirpated the visual cortex in certain dogs. It was then found that those animals which had been operated on were no longer able to discriminate as before between lights of different colours. Moreover the only light stimulus which proved effective in eliciting from dogs in this condition a difference of reaction was one which constituted either an increase or a diminution in the intensity of the light; in the former case the dogs would seize the meat, in the latter turn away from it[1]. Kalischer concludes that for colour discrimination the cortical centres must remain intact but that brightness discrimination may result from the sub-cortical centres alone.

Analogies, such as Hughlings Jackson's famous theory of 'levels,' at once suggest themselves. For the various strata postulated in the present case are not mere artificial abstractions but, like Jackson's 'levels,' may actually be said to correspond in some way to the order of evolutional development as well as to that of importance and significance for an animal's welfare.

It does, however, seem surprising that, in the case of a species possessing the relatively high powers of discrimination shown by dogs, colour and brightness differences should prove so apparently ineffectual and unimportant as contrasted with the influence of position. But recent observations show that dogs are not peculiar in this respect. Yerkes and Waugh have both demonstrated the fundamental rôle assumed by position in the mental life of the dancing mouse. The former writes, that certain facts which he has described "conclusively prove that the habit of turning in a certain direction or of choosing by position can be formed more readily than a habit which depends upon visual discrimination[2]." And in a paper by Waugh the following passage occurs: "Observation indicates that in the mouse, the visual stimuli are not of so great importance in guiding the animal as the kinaesthetic. The latter is relied upon wherever possible...Training a mouse to discriminate always involves training him away from a reliance upon the muscular sense[1]." And even more interesting for purposes of comparison with the present case are the somewhat similar phenomena observed in monkeys by Watson, who gives the following description: "The onset of position habits in the case of both H and B in the red-green test, and in that of J in the blue-yellow and the constant struggle of the experimenter to keep such position habits from forming are significant. Although these animals had been obtaining food for several days with a given color stimulus, we find instead of a steady and rapid increase in the ability to associate the color with the food, a total break in the process, and a resort, on their part, to the use of a sensory process genetically lower than the visual, namely the kinaesthetic. Such behaviour is suggestive of the relatively unimportant rôle which color vision plays in the life of this animal[2]." In view of these facts it would seem that the part played by kinaesthetic sensations in the mental life of the higher animals has hitherto been underestimated and that its importance may be, probably is, very wide-reaching indeed.

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[1] *Op. cit.* 321–2.

[2] *Op. cit.* 110.

XII. INDIVIDUAL DIFFERENCES.

From the unstable nature of the colour sense it was to be expected that the subjects would exhibit marked individual differences as regards colour discrimination, an expectation which is fully confirmed by the results of the experiments. Subject J proved much more appreciative of colour and brightness differences than the other animals tested, while subject T, as judged by his behaviour, appeared to occupy in this respect a position intermediate between subject J and subject R though rather nearer the latter.

Graber[3] and Kalischer[4] have also laid stress on the marked individual differences which dogs manifest in respect to colour (?or brightness) appreciation.

In addition to individual differences it is possible that specific variations may also be shown to exist among different breeds of dogs; a breed which like the bloodhound relies mainly on scent possessing in all probability less power of colour and brightness discrimination than one like the whippet which relies to a greater extent on sight.

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[1] "The rôle of vision in the mental life of the mouse," *J. of Comp. Neur. and Psychol.* 1910, xx. 591.

[2] *Op. cit.* 19–20.

[3] *Op. cit.* 75.

[4] *Op. cit.* 320–1.

These differences, together with dissimilarity of methods, would suffice to account for the divergence between the results recorded by various investigators.

XIII. GENERAL CONCLUSIONS.

1. That while evidence has been obtained to show that some dogs do possess a rudimentary power of colour discrimination, such discrimination is highly unstable and cannot be supposed to play any part in the animals' normal existence.
2. That the colour sense is, as shown by the high colour threshold, very weak.
3. That colour discrimination, even where clearly established, may be readily inhibited by differences of luminosity or position[1].

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[1] Samojloff and Pheophilaktowa (*op. cit.* 189) and Colvin and Burford (*op. cit.* 44) have both further shown that colour discrimination may also be inhibited by differences of *form*, their subjects apparently proving unable to recognise the food-colour when abstracted from its accustomed conditions. Orbeli, while unable to obtain from his subjects definite conditioned reflexes in the case of differences of hue, was yet able to induce them with differences of form (*op. cit.* 76 *seq.*).

APPENDIX.—TABLES.

EXPLANATION OF TABLES.

Table I gives in a summarised form the results of the 'approximate brightness value' series A and of the 'grey difference threshold' series A and B. Under each of the grey variables in the column headed '*T*' is recorded the total number of marks (as calculated by the 'graded' method, page 144) obtained in any colour or grey series for the discrimination of the particular grey from the standard (colour or grey) in question. In the companion columns headed '°/₀' is recorded the 'percentage' marks (as calculated by the 'percentage' method, page 145) corresponding thereto. The 'S_T' column gives for each series the sum of the numbers contained in the various '*T*' columns; the 'Av. °/₀' column, the average of the percentages contained in the '°/₀' columns. Under 'Analysis of reactions' in the four columns headed respectively + +, +, ?, –, is shown the exact number of times that each of these four types of reaction (page 145) occurred in the various series.

Table II gives in tabular form the results of the 'repeat brightness value' series. The columns headed '*T*' and '°/₀' show respectively the number of 'graded' marks and the percentage obtained each day for the discrimination of the variable in question from the standard colour of the series. 'Group' method and 'dispersed' method (page 140) refer to the method adopted each day in regard to the order of presentation of the variables. The numbers contained in the columns headed 'total number of marks' are obtained by adding together the totals of the '*T*' columns of the series in question for the two days on which (*a*) the 'group' method, (*b*) the 'dispersed' method, were employed. The 'graded' and 'percentage' measures of improvement express the difference, for the 'S_T' and the 'Av. °/₀' respectively, of the marks obtained for the discrimination of corresponding sets of standards and variables in this series and in the 'approximate brightness value' series A[1]. For the rest, the table is constructed in a manner similar to that described in the case of table I.

Table III. In table III similar data are recorded for the 'approximate brightness value' series B. There will be found in addition in the case of certain variables two extra columns headed 'degree of improvement.' The numbers contained in these columns ('graded,' 'percentage') express the amount of improvement, according to the 'graded' and the 'percentage' methods respectively, shown by the results of this series over those of the last four days of series A; those in the 'graded' column are obtained by halving[2] the marks given in the '*T*' column of the variable in question and subtracting therefrom the sum total marks gained for the

discrimination of corresponding pairs of standards and variables in series A; those in the 'percentage' column by treating the marks of the '°/₀' column in a similar manner. The sign '−' before a number in either the 'graded' or the 'percentage' columns indicates that the advantage lies with series A, in other words that there is an actual loss of improvement.

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[1] In order that the two series should be rendered strictly comparable the results of the last four days only of series A are considered.

[2] This process was rendered necessary owing to the fact that the number of daily tests with each variable in series B was double that of series A.

Table IV gives a summary of the results obtained in the 'hue discrimination' series A. The numbers contained in the columns headed respectively 'percentage of right cases' and 'graded marks' are obtained (according to the methods described on pages 144—146) from the data recorded in the columns headed 'analysis of reactions.' An additional column '0' is included under 'analysis of reactions,' but for purposes of calculation the cases which it contains are reckoned as + + cases (page 145). The reason of the inclusion of the '0' cases in this table and table V is due to (i) their unusual frequency, (ii) the fact that the subject's behaviour indicated positive acceptance and not mere indifference, (iii) the absence of − cases, pointing to the positive significance of the '0' reactions.

Table V gives the results obtained by subject J both in the training series as well as in the critical test (dispersed method) of the hue discrimination series B. Under the columns headed 'variable' and 'standard' are recorded the tints to which, during the tests in question, the negative response (rejection) and the positive response (acceptance), respectively, should have been given. The column headed 'no. of tests' shows the number of preliminary tests during which no definite discriminative ability was manifest; the column headed 'improvement' shows the number of the test at which definite improvement began to be manifested, while the second number shows at what test such improvement ceased to be manifest.

The scheme of letters used to designate the various tints is as follows:—*A* indicated a card of the same hue as but very slightly darker and somewhat less saturated than the card of maximum saturation of the series in question; B in turn was very slightly darker and less saturated than A, C than B, and so forth. Z, on the other hand, indicated a card of the same hue as but slightly brighter and somewhat less saturated than the maximum card of the series; Y again was brighter and less saturated than Z, X than Y, etc.

For the rest the table is constructed in a plan similar to that adopted in the case of table IV.

Table VI gives in condensed form the results obtained in the 'colour threshold' tests, together with the amount of daily improvement (page 171) shown both in that series and in the 'grey difference threshold' series B. 'First half' and 'second half' refer respectively to the two groups into which each day's tests were divided (pages 142, 143). The letters in the column headed 'tint' refer to the scheme already explained (table V). The figures in the columns headed 'no. of days' show the length of time required to establish discrimination of the tint in question. The average percentage of right cases is obtained in the accustomed manner (page 145). Under 'highest percentage' is shown the highest percentage obtained on any day for the discrimination of each tint from the standard (white) during the period of the tests. The 'repeat average °/₀' and the 'repeat highest °/₀' refer to the results obtained on the last two days of the tests when the tints originally presented were re-tested. The 'general average' consists of the average of the numbers contained in the 'average °/₀' column; the 'av. of the gen. avs.' that of the numbers contained in the two columns headed 'general average.' The 'degree of improvement' is obtained by subtracting the numbers contained in the first 'general average' column from those contained in the second 'general average column.' In the case of the 'grey difference threshold' series B the 'average °/₀,' the 'percentage improvement' and the 'av. of gen. avs.' were obtained in a similar manner. The columns headed 'total graded marks' give the total marks (as calculated by the 'graded' method, page 144) obtained in each daily half for the discrimination of the standard (grey 30) from the six variables; the 'graded improvement' consists of the difference between these two sets of marks.

[Tables I—VI follow]

(*Manuscript received March* 23, 1912.)

TABLE I. ‘*Approximate brightness*

Subject	Colour of standard	Grey 1		Grey 5		Grey 10		Grey 15		Grey 20		Grey 25	
		T	°/ₒ	*T*	°/ₒ	*T*	°/ₒ	*T*	°/ₒ	*T*	°/ₒ	*T*	°/ₒ
T	Blue, March 21—26, 1910	48	100	36	90	26	83·3̇	33	86·6̇	23	80	- 6	46·6̇
J	Blue, Feb. 8—13, 1910	38	93·3̇	40	93·3̇	46	100	40	96·6̇	37	86·6̇	20	73·3̇
	Red, Feb. 23—28, 1910	56	100	52	100	50	100	39	93·3̇	29	80	22	70
	Yellow, March 18—24, 1910	24	73·3	44	93·3̇	53	100	58	100	56	100	56	100
	Green, March 31—April 25—29, 1910	39	90	46	93·3̇	55	100	57	100	54	100	58	100
R*	Green, Jan. 7—13, 1910	1	54·16̇	23	83·3̇	40	100	36	91·6̇	43	100	44	95·83̇

‘*Grey difference*

		Grey 1		Grey 5		Grey 10		Grey 15		Grey 20		Grey 25	
J	Grey 30, 1st six days, Oct. 21—27, 1910	31	76·6̇	12	60	2	56·6̇	15	63·3̇	- 6	50	0	44
	2nd six days, Oct. 28—Nov. 3, 1910	58	100	37	80	16	70	11	60	15	63·3̇	13	66·6̇

‘*Grey difference*

J	Grey 30, 1st four days, Nov. 4—8, 1910							17	60	14	57·5	- 3	47·5
	2nd six days, Nov. 9—14, 1910							51	85	14	60	13	60

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* It should be noted that subject R was given only four tests a day with each grey, making a total of forty-four tests daily in all; whereas the other subjects were given five tests a day with each grey—*i.e.* fifty-five tests in all, each day.

value' series A.

Grey 30		Grey 35		Grey 40		Grey 45		Grey 50		Analysis of reactions				S_T	Av. °/ₒ
T	°/ₒ	*T*	°/ₒ	*T*	°/ₒ	*T*	°/ₒ	*T*	°/ₒ	+ +	+	?	–		
32	90	30	90	27	83·3̇	28	83·3̇	28	83·3̇	95	180	47	8	305	83·3̇
9	60	26	80	50	100	43	96·6̇	43	96·6̇	150	143	23	14	387	89
25	80	30	80	41	96·6̇	44	100	42	96·6̇	164	135	29	2	480	90·5
58	100	57	100	55	100	56	96·6̇	57	100	267	52	10	1	574	96·6̇3̇
59	100	58	100	60	100	60	100	60	100	286	39	5	0	608	98·4̇5̇
43	100	48	100	45	100	44	100	47	100	192	54	12	6	414	93·1̇8̇

threshold' series A.

Grey 25$_o$		Grey 35		Grey 40		Grey 45		Grey 50							
–16	30	–13	43·3̇	4	53·3̇	0	50	21	70	136	39	23	117	52	54·3̇0̇
–11	40	7	63·3̇	10	56·6̇	15	70	35	83·3̇	159	69	23	79	206	68·4̇8̇

threshold' series B.

–6	50	16	62·5	–8	45					95	34	28	83	30	53·749̇
–12	42·5	20	62·5	16	62·5					116	33	19	72	102	62·08

TABLE II. '*Repeat brightness value*' *series.*

Subject	Colour	No. of grey	1st day Group method		2nd day Dispersed method		3rd day Group method		4th day Dispersed method		Analysis of reactions				Total no. of marks		Average %		S_T	Av. %	Degree of improvement	
			T	%	T	%	T	%	T	%	++	+	?	-	Group method	Dispersed method	Group method	Dispersed method			Graded	Percentage
J	Blue (May 7, 9, 17, 24, 1910)	1	4	80	6	100	8	100	6	80	8	10	2						24	90		
		5	6	80	5	100	9	100	6	80	10	8	2						26	90		
		10	6	100	4	80	6	100	4	80	4	14	2						20	90		
		15	7	100	9	100	8	100	2	60	11	7	1	1					26	90		
		20	2	60	3	80	9	100	6	80	8	8	4						20	80		
		25	5	80	4	80	4	60	5	80	8	7	5						18	75		
		30	4	80	7	100	0	40	8	100	7	9	4						19	80		
		35	-1	40	5	80	1	60	9	100	8	6	4	2					14	70		
		40	8	100	9	100	7	100	9	100	13	7							33	100		
		45	8	100	6	80	8	100	9	100	13	6	1						31	95		
		50	9	100	8	100	9	100	8	100	14	6							34	100		
			58	83·6̇3̇	66	90·9̇0̇	69	87·2̇7̇	72	87·2̇7̇	104	88	25	3	127	138	85·4̇5̇	89·0̇9̇	265	87·25	-14	-4·25
	Red (May 5, 12, 14, 26, 1910)	1	5	80	9	100	4	80	3	60	9	7	4						21	80		
		5	8	100	8	100	-1	40	8	100	9	8	3						23	85		
		10	8	100	8	100	-1	40	6	100	7	10	3						21	85		
		15	9	100	3	80	9	100	1	40	10	6	4						22	80		
		20	9	100	8	100	6	80	2	60	11	6	3						25	85		
		25	9	100	2	60	6	80	6	80	11	5	4						23	80		
		30	9	100	7	100	6	80	9	100	13	6	1						31	95		
		35	7	100	5	80	7	100	7	100	8	11	1						26	95		
		40	9	100	9	100	8	100	10	100	16	4							36	100		
		45	8	100	8	100	9	100	5	80	12	7	1						30	95		
		50	8	100	8	100	7	100	9	100	12	8							32	100		
			89	98·1̇8̇	75	92·7̇2̇	60	81·8̇1̇	66	83·6̇3̇	118	78	24		149	141	89·9̇	88·1̇8̇	290	89·25	-6	-2
	Yellow (May 6, 10, 19, 20, 1910)	1	6	80	6	80	4	60	1	60	9	5	6						17	70		
		5	9	100	0	40	5	80	6	80	10	5	5						20	75		
		10	9	100	9	100	9	100	10	100	17	3							37	100		
		15	8	100	10	100	10	100	10	100	18	2							38	100		
		20	10	100	7	100	10	100	10	100	17	3							37	100		
		25	10	100	8	100	10	100	9	100	17	3							37	100		
		30	10	100	10	100	9	100	10	100	19	1							39	100		
		35	9	100	9	100	10	100	9	100	17	3							37	100		
		40	10	100	10	100	9	100	10	100	19	1							39	100		
		45	10	100	9	100	10	100	10	100	19	1							39	100		
		50	10	100	10	100	10	100	10	100	20								40	100		
			101	98·1̇8̇	88	92·7̇2̇	96	94·5̇4̇	95	94·5̇4̇	182	27	11		197	183	96·3̇6̇	93·6̇3̇	380	95·0	-18	-1·75

	Green (Apr. 29, May 13, 16, 25, 1910)	1	6	80	7	100	5	80	1	40	9	6	5						19	75		
		5	6	80	5	80	7	100	6	80	10	7	3						24	85		
		10	10	100	7	100	9	100	9	100	15	5							35	100		
		15	10	100	10	100	8	100	9	100	17	3							37	100		
		20	10	100	9	100	8	100	9	100	16	4							36	100		
		25	10	100	8	100	8	100	10	100	16	4							36	100		
		30	10	100	9	100	8	100	10	100	17	3							37	100		
		35	10	100	10	100	10	100	10	100	20								40	100		
		40	10	100	9	100	10	100	9	100	18	2							38	100		
		45	10	100	10	100	10	100	10	100	20								40	100		
		50	10	100	10	100	10	100	9	100	19	1							39	100		
			102	96·3̇6̇	94	98·1̇8̇	93	98·1̇8̇	92	92·7̇2̇	177	35	8		195	186	97·2̇7̇	95·4̇5̇	381	96·25	-25	-1·75
			350	96·3̇6̇	323	93·6̇3̇	318	90·4̇5̇	315	89·54					668	648	92·2̇7̇	91·59̇0̇				

Subject	Colour	No. of grey	Group method								Analysis of reactions					Av. %
			1st day		2nd day		3rd day		4th day							
			T	%	*T*	%	*T*	%	*T*	%	+ +	+	?	-	0*	
R	Green (July 5, 6, 7, 8, 1910)	1	1	60	-3	40	6	83·3̇	28	100	12	6	3	3	7	60·5
		5	-3	20	-1	60	11	100	23	86·6̇	13	5	5	1	7	58·8
		10	9	100	9	100	7	83·3̇	25	93·3̇	19	5	1	1	5	94·4
		15	-1	40	4	80	9	83·3̇	27	100	11	7	4	1	8	65·5̇
		20	9	100	6	100	12	100			9	5			2	100
		25	2	60	9	100	11	100			8	3	1	1	3	88·8̇
		30	8	100	8	100	11	100			8	5			3	100
		35	2	60	8	100	11	100			7	5	2		2	86·1̇
		40	8	100	8	100	12	100			9	4			3	100
		45	5	80	10	100	12	100			12	2	1		1	94·4̇
		50	5	80	8	100	12	100			9	4	1		2	94·4̇
			45	72·7̇2̇	66	89·0̇9̇	114	95·4̇5̇	103	94 9̇	117	51	18	7	43	76·645...

* In R's case the '0' reactions were so frequent (page 180) that it was considered advisable to take them into account.

TABLE III. '*Approximate brightness value*' *series B.*

Subject	Colour of standard	*T*	°/o	Degree of improvement		*T*	°/o	*T*	°/o	Degree of improvement		*T*	°/o	*T*	°/o	Degree of improvement		*T*	°/o	Analysis of reactions				S_r	Av. °/o
				Graded	Per-centage					Graded	Per-centage					Graded	Per-centage			+ +	+	?	-		
		Grey 20				Grey 23		Grey 25				Grey 28		Grey 30											
T	Blue, March 26, 29—31, 1910	39	80	2·5	- 5	41	80	26	67·5	13·5	12·5	51	92·2	49	90	5·5	5			82	82	29	7	205	82
		Grey 25				Grey 27		Grey 30				Grey 33		Grey 35											
J	Blue, February 16—19, 1910	53	85	6·5	5	71	100	52	85	17	20	67	97·5	71	100	6·5	10			140	47	13		314	93·5
		Grey 20				Grey 23		Grey 25				Grey 28		Grey 30											
	Red, March 2—5, 1910	59	100	8·5	·15	56	92·5	30	70	1	0	56	92·5	64	97·5	15	17·5			103	78	19		265	90·5
		Grey 1				Grey 2		Grey 3				Grey 4		Grey 5											
	Yellow, March 26—29, 1910	91	85	11·5	·15	51	85	51	90			68	100	63	100	·5	·5			117	67	15	1	284	92
	Green, April 30, May 2—4, 1910	44	87·5	- 5	- 5	60	92·5	61·95				70	100	60	90	0	5			122	65	12	1	295	93
																		Yellow							
R	Green, June 14—21, 1910	- 7	42·5			21	60	- 2	48·75			15	72·5	11	71·25			- 47	31·226	41	55	57	51	19	59

TABLE IV. '*Hue discrimination*' *series A*.

Subject	Date	Standard (positive)	Analysis of reactions					Percentage of right cases	Graded marks	Remarks
			+ +	+	?	–	0			
J	May 30, 1910	All four colours (group method) 5 tests each	10	3	1 (blue)		6	95	34	
		Same (dispersed method)	6	8	4 (blue 1, red 1, yellow 1, green 1)		2	80	20	
	May 31	,, ,,	21	11	6 (blue 4, yellow 1, green 1)		2	85	49	Rejection method of selection. Very careful and prolonged comparison.
	June 1	,, ,,	15	10	8 (blue or red, 1 yellow)	1	6	77·5	42	
	,, 2	,, ,,	17	11	1 (red)		3	96·87	50	
	,, 4	,, ,,	21	15	4 (yellow 2, blue 1, red 1)			90	53	
	,, 6	,, ,,	5	8	7 (blue 3, red 1, yellow 2, green 1)			65	11	Very inattentive and indifferent.
	,, 7	,, ,,	21	11	7 (blue 4, red 2, yellow 1)	1 (green)		80	44	Hesitating and uncertain: tedious comparisons.

TABLE V. Subject J. '*Hue discrimination*' *series B.*

Date	Standard (positive)	Variable (negative)	No. of tests	Improvement	Analysis of reactions					Percentage of right cases	Remarks
					+ +	+	?	-	0*		
Training tests											
July 14	Blue A (1)	Red A—B†	1—6		1		1	2	2	50	
				7—23	9	1	2		5	88·35	
,,	,,	Red B	1—10		3	3		1	3	90	
,,	Zimmermann's Blue and Red. (Red slightly brighter than Red B)		1—7		5	1			1	100	Very definite and careful.
,,	Blue A	Red F	1—5		3	1	1 (1)			80	
,,	Blue X	Red T	1—5		3	2				100	
,,	Blue C	Red X	1—5		5					100	
,,	Blue Z	Red F	1—6		4				2	100	
,,	Blue F	Red G	1—7		4	1	1		1	85·71...	
,,	Blue X	Red T	1—5		4	1				100	
July 15	Zimmermann's max.		1—5		3				2	100	
,,	Blue A	Red A	1—8		2	1	1	2	2	62·5	
				9—14	4		1		1	83·3̇	
,,	Blue X	Red T	1—10		8	2				100	
,,	Blue F	Red G	1—10		6	2		1 (1)	1	90	
,,	Blue Z	Red E	1—5		4				1	100	
,,	Blue Y	Red D	1—5		4		1			80	
,,	Blue A	Red A	1—6		5	1				100	
July 16	,,	,,	1—24		10		4	3	7	70·83...	
Dispersed method											
,,	Blue and Red, fifteen very different pairs		1—30		21	3		1	5	96·6	
,,	Blues with Orange, Magenta, Yellows and Red A (max.)		1—10		5		1	1	3	80	Seems evident that colour plays some part.
Training tests											
July 19	Blue B	Yellow H	1—8		2	1	3		2	62·5	
				9—24	10	3	2		1	87·5	
,,	Blue Z	,,	1—10		5	3			2	100	Repulsive power of yellow greater than attractive power of blue.
,,	Blue X	,,	1—22		12	2	3	4	1	68·40...	
,,	Blue W	,,	1—10		6	3			1	100	

July 20	Blue V	,,	1—19		6	4	2	4	3	68·42...	Much more difficult than light blue and pink. Cf. red (max.) and blue (max.) at start, page 162.
				20—29	7	1			2	100	
,,	Blue U	,,	1—20		7	4	4	2	3	70	
,,	Blue X	,,	1—14		7	4	2	1		78·57	
				15—26	7	5				100	
,,	Blue Y	,,	1—10		6	4				100	
July 21	Blue V	,,	1—32		14	4	2	7	5	71·56...	J pays no attention. Very bad.
July 22	Red (max.)	Green G	1—6		4	2				100	
,,	Red A	,,	1—5		3				2	100	Red has repulsive force.
,,	Red Z	,,	1—5		3	1			1	100	
,,	Green G	Red Y	1—4		2			2		50	
				5—14	9				1	100	
,,	,,	Red X	1—8		5	1	1		1	87·5	
,,	,,	Red W	1—6		4	1	1			83·3̇	
,,	,,	Red V	1—4		2			2		50	
				5—9	4	1				100	
,,	,,	Red U	1—13		9	2		2		74·61	
July 23	,,	Red T	1—56		19	10	3	20	4	55·33...	J careful. Discrimination very difficult.
				57—68	9	2	1			91·6̇	
July 24	Green F	,,	1—19		9	5	1	4		73·68	No positive acceptance for green to-day. Rejection of red very definite and careful.
				20—36	12	4		1		94·12	
,,	Green E	,,	1—10		9	1				100	Very careful consideration and comparison.
July 25	Green D 3	,,	1—13		6	1		5	1	61·53...	Pink seems especially difficult to see from a distance.
				14—28	11	3		1		93·3̇	
,,	Green D 2	,,	1—10		4	2	1	3		60	
				11—26	9	6		1		93·75	
,,	Green D	,,	1-15		9	5		1		93·3̇	
July 26	Green C	,,	1—8			4		4		50	Rejection of red very definite.
				9—31	18	2	1	2		86·95...	
,,	Green B	,,	1—15		8	6		1		93·3̇	
,,	Green (max.)	,,	1—10		6	4				100	Colours more difficult to discriminate from a distance than most.
July 27	Green W	,,	1—15		11	2		1	1	93·3̇	
				Dispersed method—Green (+) Red (−)							
,,	Green and Red, 22 very different pairs		1—50		29	10		7	4	86	
July 28	Green and Red, 19 pairs		1—30		18	6		6		80	Very careless and inattentive.
July 29	Green and Red, 29 pairs		1—23		11	3	1	6	2	69·56...	J's mood execrable—no interest, care or comparison.
				24—50	17	6		2	2	92·59...	Improvement very marked.
July 30	Green and Red, 34 pairs		1—50		32	6		2	10	96	No hesitation. Very certain and definite. Notice number of 0 cases.

* See explanation of table III.

† This card was intermediate in tint between red A and red B.

TABLE VI

Subject	Standard	Variable	Tint	1st half					General average	2nd half					General average	Degree of improvement	Av. of general averages
				No. of days	Av. %	Highest %	'Repeat' av. %	'Repeat' highest %		No. of days	Av. %	Highest %	'Repeat' av. %	'Repeat' highest %			
J	White (Nov. 16 to Dec. 18, 1910)	Red	T	10	57	70	65	70	63·448...	8	77·5	90	85	100	67	3·552...	65·224...
			T_x	1	90	90				1	80	80					
			T_{xx}	5	70	80				5	72	80					
			T_y	11	63·6̇3̇	80				11	52·7̇2̇	70					
		Blue	W	10	58	90	60	70	58·272...	8	71·25	80	55	60	64·4̇...	6·172...	61·368...
			V	6	70	90				6	63·3̇	80					
			U	11	51·8̇1̇	70				11	61·8̇1̇	80					
		Green	Z	10	64	90	55	70	61·79...	8	62·5	90	70	90	65·5̇	3·765	63·672̇...
			Y	6	71·6	90				6	66·6̇	90					
			W	11	55·45	90				11	66·3̇6̇	90					
		Yellow	Z	16	60	80	60	70	59·65...	14	62·85	90	55	60	59·62...	0	59·63...
			V	11	59·0̇9̇	80				11	56·3̇6̇	80					
		Grey	5	9	62·2	100	80	90	65	8	68·75	90	95	100	67·7̇	2·7̇	66·35
			4	1	90	90				1	80	80					
			3	8	73·7̇5̇	90				8	61·25	80					
			2	8	52·5	70				8	65	80					

'Grey difference threshold' series B.

Subject		1st half		2nd half		'Graded' improvement	'Percentage' improvement	Av. of general averages
		Total 'graded' marks	Av. %	Total 'graded' marks	Av. %			
J	Grey 30, 1st four days, Nov. 4—8, 1910	5	51·6̇	25	55·8̇3̇	20	4·16̇	53·75̇2̇
	2nd four days, Nov. 9—14, 1910	42	60	60	64·16	18	4·16̇	62·0̇8̇

A COMPARISON OF PSYCHOPHYSICAL METHODS.

BY GODFREY H. THOMSON.

I. Methods and Processes.
II. The Experiments.
III. Processes of Calculation.
IV. Theory of the Group Process.
V. Calculation of Probable Errors.
VI. Comparison of the Methods of Right and Wrong Cases and Minimal Changes.
VII. Comparison of the Methods of Right and Wrong Cases and Serial Groups.
VIII. Comparison of the Limiting and Group Processes.
IX. Comparison of the Limiting and Constant Processes.
X. An Improved Form of Totalling in the Constant Process.
XI. The Psychometric Function.
XII. Changes in the Threshold during a Sitting.
XIII. Introspection of the Subjects.
XIV. Summary.

I. METHODS AND PROCESSES.

THE following investigation, undertaken at the suggestion of Dr C. S. Myers, was begun in the Cambridge Psychological Laboratory in July 1911, and continued at Armstrong College, Newcastle-on-Tyne, during the following Michaelmas and Epiphany Terms. Its object was to compare various psychophysical methods of measuring the cutaneous spatial threshold.

Now there are two things, essentially different from one another, which are commonly confused under the one heading 'psychophysical methods,' namely the *methods* of experimenting in order to obtain data, and the *processes* of calculation after the data have been collected. To avoid this confusion the words 'method' and 'process' will be employed throughout this paper in the way indicated by their use in the above sentence. It is urged that their general adoption would be advantageous. The cause of the confusion is to be found in the historical development of the subject, for with each of the methods of experimenting a process of calculation was associated and the one name was given to both.

The experimental methods of determining thresholds may be divided into two main groups:

1. Methods in which the stimulus is altered continuously until in the opinion of the subject it fulfils some given condition.
2. Methods in which various values of the stimulus are separately submitted by the experimenter to the subject who expresses a judgment on each of them, classifying them into two or more categories.

This paper is concerned only with the methods belonging to the second group. These methods may in turn be classified according to the order in which the stimuli are submitted to the subject. The second group is thus subdivided as follows:

2(*a*). Methods in which the order of succession of the stimuli is irregular, non-consecutive.

2(*b*). Methods in which the order of succession of the stimuli is consecutively (i) ascending or (ii) descending.

Under 2(*a*) comes the Method of Right and Wrong Cases. Under 2(*b*) come the Method of Minimal Changes and the Method of Serial Groups[1]. The latter is a special case of the Method of Minimal Changes, in which each value of the stimulus is submitted a number of times before the next consecutive value is submitted. A corresponding method (not hitherto employed) under 2(*a*) would be the Method of Non-Consecutive Groups, in which groups would be taken in an irregular order. Experiments by this method are described later. It is customary in the Method of Serial Groups to introduce among the stimuli an equal number of 'catch' cases, in which no stimulus

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[1] I adopt here the convenient name suggested by G. M. Stratton who first described the method (*Psychol. Rev.* 1902, IX. 444—447). It had, however, been independently discovered and employed by W. McDougall four years previously during his stay in Murray Island (*Rep. Cambridge Anthrop. Expedition to Torres Straits*, Cambridge, 1903, II. 190—193).

(or stimulus difference) at all is given. This might also be done in other methods. Each method can be further subdivided according as the subject is or is not warned beforehand of the kind of succession to expect.

The differences which result from the use of these various 'methods' are clearly due to their different psychological effects upon the subject. But we have also to take into consideration the differences in the 'processes' of calculation. These, of course, are purely mathematical and ignore the subject altogether.

Now, it is evident at the outset that the Method of Minimal Changes may be expected to prove less reliable than the other methods, because of the influence of expectation, even although the point of starting and the size of the steps be changed frequently.

Of the other methods, the Method of Right and Wrong Cases gives twice as many results from the same number of judgments as the Method of Groups, because, in the latter, half the judgments are 'catches.' On the other hand, the Method of Groups collects judgments much more quickly, because the instrument only needs to be set between the Groups. Care must be taken, however, not to present a stimulus too soon after the preceding one. Indeed, with some subjects such necessity for caution might counterbalance the advantage of the rapidity of the method.

The Method of Groups gives an indication of unusual conditions by the number of 'catch errors'[1] observed; this is a great advantage. This method is more readily understood by children and primitive races. Moreover, the possibility of being 'caught out' induces care.

Although the experiments now to be described and discussed are far from conclusive, it is the opinion of the writer that the Method of Groups is the most practical. For rapid and approximate determinations on many subjects or on many occasions the Method of Serial Groups is the quickest and safest; it takes the place of the Method of Minimal Changes of which it is a special form. But for the more careful collection of data the Method of Non-Consecutive Groups is suggested instead of the Method of Right and Wrong Cases, because it includes 'catch' judgments without increasing the time. In this method also it is easier to keep the interval between judgments constant, which is found to be an advantage. The data can be handled by any of the mathematical 'processes.'

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[1] I use this term for the wrong answers given by the subject when 'catches' are presented.

II. The Experiments.

All the subjects were given two days' preliminary practice. My sincere thanks are due to them for so willingly devoting their time to the experiments.

The instrument used in these experiments was an ordinary pair of dividers from a box of drawing instruments. The limbs were about four inches long. The subjects put the right arm through a loose screen of cloth so that the forearm rested comfortably on a desk, with the palm downward. The right hand of the experimenter, who sat facing the subject, held the aesthesiometer and rested on a pile of three books. A similar portion of each arm was chosen, the fleshy part on the inner-upper surface of the forearm as it lies in the position indicated, nearer the elbow than the wrist. The exact area was recognised by veins and markings. The two points were always placed longitudinally. The pressure of the points was kept as constant as possible, in order to secure which visual observation of the dimples formed in the skin was found to be of assistance. The judgment was given before the instrument was removed.

In all, six subjects were experimented upon. The experiments on No. 1, a boy of 14½, were preliminary experiments only, while those on No. 6 were designed for a special purpose and are described on page 214. The four subjects Nos. 2, 3, 4 and 5 were aged 20, 30, 21 and 22 years respectively and were all well-educated men.

Subjects 2 and 3 were 'uninformed,' subjects 4 and 5 were 'informed.' That is to say, subjects 2 and 3 were unaware of the objects of the experiments and were told nothing about the order of the stimuli. Cautious questions during the course and fuller questioning at the close of the experiments showed that they had not noticed that the order was sometimes regular, and sometimes irregular. On being questioned, No. 3 said he had noticed that sometimes successive stimuli came more quickly (Serial Groups) than at other times; he also suggested as a possible difference that "on some days you appeared to press the compass points down more."

Subject No. 4 was at first uninformed but later detected the Method of Minimal Changes, and was therefore informed. Consequently, this subject and No. 5 knew that certain methods (which were explained to them) were to be compared, and they knew which was to be used at each sitting. They did not know, however, the range, nor

TABLE I. *Methods used at each Sitting.*

Subject No. 2					Subject No. 3					Subject No. 4					Subject No. 5					Subject No. 6
Time and Date	R. and W. cases	S. G. descending	S. G. ascending	Minimal changes	Time and Date	R. and W. cases	S. G. descending	S. G. ascending	Minimal changes	Time and Date	R. and W. cases	S. G. descending	S. G. ascending	Minimal changes	Time and Date	R. and W. cases	S. G. descending	S. G. ascending	Minimal changes	Method of Non-Consecutive Groups only. Time and Date
11.18, Aug. 24	—	—	—	1	5.21, Oct. 11	3	—	2	1	11.20, Aug. 21	1	—	—	—	10.22, Sept. 1	1	—	2	—	8.50, Mar. 4
11.28, ,, 26	1	—	—	—	5.27, ,, 13	1	2	—	3	11.21, ,, 22	—	1	—	—	10.30, ,, 2	2	—	—	1	,, ,, 5
11.30, ,, 29	—	—	1	—	5.35, ,, 18	3	2	1	—	11.31, ,, 24	—	—	—	1	10.21, ,, 5	—	1	—	2	,, ,, 7
11.6, ,, 31	—	—	—	1	5.34, ,, 20	—	1	2	3	11.12, ,, 26	1	—	—	—	10.5, ,, 6	—	—	1	2	,, ,, 8
10.58, Sept. 1	1	2	—	—	5.20, ,, 24	1	—	—	2	11.20, ,, 29	—	—	1	—	10.12, ,, 8	2	—	—	1	,, ,, 11
11.12, ,, 2	2	—	—	1	5.55, Nov. 13	1	—	2	—	11.21, ,, 31	—	—	—	1	10.30, Nov. 6	2	—	*1	—	,, ,, 12
11.23, ,, 4	—	—	1	2	5.37, Dec. 7	2	—	3	1	11.11, Sept. 1	1	2	—	—	11.18, ,, 10	—	2	—	1	,, ,, 14
11.9, ,, 5	2	1	—	—	6.38, ,, 8	—	3	1	2	11.6, ,, 4	2	—	—	1	11.47, Dec. 9	1	—	—	2	,, ,, 15
10.55, ,, 6	—	—	1	2	6.48, ,, 19	—	2	3	1	11.20, ,, 5	—	—	1	2	10.15, ,, 11	1	—	—	2	,, ,, 19
9.20, Oct. 20	—	1	2	—	5.54, ,, 21	—	—	1	2	11.4, ,, 6	—	—	1	2						,, ,, 20
9.22, ,, 25	—	—	2	1						10.50, ,, 8	2	1	—	—						,, ,, 21
9.17, Nov. 1	1	—	—	2						4.32, Nov. 16	2	—	1	—						,, ,, 22
9.30, ,, 3	—	2	1	—						12.3, ,, 18	1	—	—	2						,, ,, 25
10.15, Dec. 9	—	—	2	1						4.35, ,, 30	—	—	*2	1						,, ,, 26
										11.57, Dec. 2	1	—	*2	—						
										11.55, ,, 16	—	—	2	1						

* Many catch errors.

were they told whether ascents or descents were being used; though this would soon be discovered[1].

The sittings for subjects 2, 4 and 5 took place in the forenoon, excepting two sittings with No. 4, which were in the afternoon. All the sittings for No. 3 took place at about 6 p.m.

At a single sitting one hundred judgments were taken—with No. 3 one hundred and fifty. Fifty were by one method and fifty by another; with five minutes rest between. Each fifty took about ten minutes; with the Method of Serial Groups only about six minutes. The order of the sittings (1, 2, etc.) is given in Table I. It would have been better had each method been used on each day. The original intention was to compare the methods daily, and then to adjust the comparisons by the theory of least squares. But this was abandoned. It is indeed doubtful whether any reliable measurement of a threshold can be made at one sitting. In any case, *one* measurement by the Method of Serial Groups gives no measure of scatter.

The subjects were asked to report whether the touch was 'two' or 'one,' and were allowed to qualify their answers by such additions as 'doubtful' and 'very doubtful.' These qualifications, though recorded, were not used in the calculations. The answers 'undecided' or 'I can't tell' or 'no judgment,' if given, were accepted without remark. They were only very seldom given. Tables II, III and IV give specimen forms.

TABLE II. *Method of Right and Wrong Cases.*

Subject 2. **Date, 2nd Sept. 1911.**
Began, 11.38 a.m. **Finished, 11.48 a.m.**

1 cm.		2 cm.		3 cm.		4 cm.		5 cm.	
e	W	*b*	W	*d*	R	*a*	R	*c*	R
b	W	*a*	R?	*c*	R	*d*	R	*e*	R
a	R?	*d*	W	*e*	R?	*c*	R	*b*	R
c	W	*e*	R?	*a*	R	*b*	R	*d*	R
d	R?	*c*	W	*b*	W	*e*	R	*a*	R
e	W	*b*	R	*d*	W	*a*	R	*c*	R
b	W	*a*	R?	*c*	W	*d*	R?	*e*	R
a	W	*d*	W	*e*	R	*c*	W??	*b*	R
c	W?	*e*	R?	*a*	R	*b*	R	*d*	R
d	W?	*c*	R	*b*	R?	*e*	R	*a*	R

The italicised letters indicate the order, which was always the same.

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[1] In the Method of Minimal Changes ascents and descents did not necessarily alternate.

TABLE III. *Method of Minimal Changes.*

Subject 3. Date, 11th Oct. 1911.
Began, 5.21 p.m. Finished, 5.33 p.m.

0 cm.	0·5 cm.	1 cm.	1·5 cm.	2 cm.	2·5 cm.	3 cm.	3·5 cm.	4 cm.	4·5 cm.	5 cm.	5·5 cm.	6 cm.	6·5 cm.	Directions
1	—	W	—	R	—	R	—	W	—	R	—	R	—	←
1	—	W?	—	W	—	W	—	W	—	R	—	R	—	→
—	W	—	?	—	R	—	W	—	R	—	R	—	R	←
—	W	—	R	—	W	—	R	—	R	—	R	—	—	→
—	W	W	R	R	W	R	R	W?	R	R	—	—	—	←
—	W	W	W	W	W	R	R	R	R	—	—	—	—	→

No pauses occurred between ascents and descents.
In this case only 46 judgments were taken. This was exceptional.

TABLE IV. *Method of Serial Groups.* Ascending.

Subject 4. Date, 5th Sept. 1911.
Began, 11.20 a.m. Finished, 11.27 a.m.

→

Kind of touch	1 cm.	2 cm.	3 cm.	4 cm.	5 cm.
1	W?	R	R??	R?	R
2	W	W	R	R	R
1	R	R	W	R	W
2	W	W	W	R	R
1	R	R	R?	R	R
2	W	W?	R	R	R
2	W	W	R	R	R
1	R	R	R	W	R
2	W	W	R	W	R
1	R?	R	R	R	R

In the Method of Serial Groups no notice was taken of occasional catch errors. If many occurred the sitting was suspended, but this occurred only thrice. It will be seen from Table IV that the experiments were continued until the fifty judgments had been given, even though the group had already been found (3 cm.) at which four correct answers were given. In the same way in the Method of Minimal Changes (Table III) a descent was continued even after a wrong answer had been given, and an ascent after a right answer. Twenty-four judgments, with distances from 1—4½ cm., would have sufficed in the specimen here given to obtain the same observations of the just perceptible and just imperceptible distances, three of each. The Method of Right and Wrong Cases (Table II) fixes ten of each with fifty judgments, and is therefore richer in results.

The experiments on subject No. 6 are described later.

III. PROCESSES OF CALCULATION.

These, again, are of two kinds. The process usually adopted with the Method of Right and Wrong Cases (which is called throughout this paper the Constant Process) and the process usually adopted with the Method of Minimal Changes (called here the Limiting Process) are typical of each. It would doubtless tend to clearness if these names became general. We have two (or more) names for each method, let us employ one of the names for the process and one for the method.

The different processes of calculation are the outcome of radically different definitions of the threshold. In the Constant Process the percentage of answers of 'twoness' at each distance[1] is obtained and from these, by making some assumption as to the mathematical form of the 'psychometric function,' the point can be found at which the probability is greatest that half the answers will be 'two.' This point is defined as the threshold. By this process, too, a measure is obtained of the sharpness of the threshold. One such measure is half the difference between the distances at which one-quarter and three-quarters of the answers will probably be 'two[2].'

In the Limiting Process a different definition of the threshold is taken. In a descent the first distance is noted at which the answer is 'not two' and in an ascent the first distance at which the answer is

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[1] For the sake of greater clearness reference is here made to measurements of a particular threshold, the cutaneous spatial threshold.

[2] A rough and ready approximation to the threshold can be obtained by finding the point D where $p=0{\cdot}5$ by linear interpolation between the two adjacent values of p. (*Vide* e.g. W. Brown, *The Essentials of Mental Measurement*, Cambridge, 1911, 23.) It may be pointed out that an approximation to the P. E. of one observation may be obtained in a similar way by finding by linear interpolation the points Q_1 and Q_2 where $p=0{\cdot}25$ and $0{\cdot}75$ respectively. Q_2-D and $D-Q_1$ are then rough measures of the P. E. of one observation above and below D respectively, and $\frac{Q_1+D+Q_2}{3}$ will approximate to the threshold found by the assumption of any symmetrical distribution, say a Gaussian. In Riecker's data quoted by Brown (*loc. cit.*), $\frac{Q_1+D+Q_2}{3}=1{\cdot}86$ Paris lines, as compared with the Fechner-Müller value $S=1{\cdot}88$ Paris lines found by Titchener (*Experimental Psychology*, 1905, II. Pt. 1, 102).

'two.' The arithmetical mean of a number of such observations is defined as the threshold. We have spoken here of ascents and descents as though the Method of Minimal Changes were being used; but, as Urban[1] has shown, the same process can be applied to data collected by the Method of Right and Wrong Cases[2]. This, however, does not for one moment mean that the Method of Minimal Changes is the same as the Method of Right and Wrong Cases; for although the *form* of P_k (see footnote 2) is not altered by a change in the order of succession of the q's, yet the *value* of P_k may very well be altered, for the q's now have, or may have, different values. Each q in fact is not only a function of its r, but also most probably of the r's which have immediately preceded.

Urban indeed unnecessarily limits the process when he says that although the order of succession of the r's in a series does not matter, yet each r of the series must be presented once before any one of them is presented a second time. If there are n values of r, each of which is to be presented m times, then if a table is made with n columns and m rows, it may be filled up in any order whatsoever, provided only that a prearranged order is adhered to and that the answers after they are given are not changed from row to row at the whim of the calculator. With this table calculations can be carried out by the Constant Process, by the Limiting Process, or by the special form of the latter used in the Serial Groups. Of the first two processes Urban has shown by experiment that the latter is more accurate than the former (when the $\Phi(y)$ or the $\tan^{-1}$ hypotheses of the psychometric function are used).

In his experiments, the object of which was to compare *processes*, Urban was careful to use the same *method*, indeed the same data, for each process. It is obvious that in the present investigation, which was designed to compare *methods*, the same *process* of calculation must be used for each method. Accordingly, the Limiting Process is used in the calculations hereafter given for comparing the Methods of Right

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[1] *Arch. f. d. ges. Psychol.* 1909, xv. xvi.

[2] The formula upon which this statement rests is

$$\text{I.} \qquad P_k = q_1 q_2 q_3 \cdots q_{k-1} p_k,$$

where P_k is the probability of the event that the stimulus r_k in an ascending series $r_1 r_2 r_3 \ldots r_k \ldots r_n$ will be noted as the *first* answer 'two,'

p_k is the probability of an answer 'two' at r_k,

$q_k = 1 - p_k$ is the probability of an answer other than 'two' at r_k.

A corresponding expression holds for descents.

This expression will not be altered in form if the order of succession of the q's (that is of the r's) be changed. That is to say, the Limiting Process can be applied to data collected by the Method of Right and Wrong Cases.

and Wrong Cases and Minimal Changes. But before deciding on the process to be used in comparing the Methods of Right and Wrong Cases and Serial Groups, it is necessary to discuss the theory of the latter method. So far as the writer is aware, this has not been heretofore attempted.

IV. THEORY OF THE GROUP PROCESS.

In the simplest form of this method each group consists of five double touches, and five single touches irregularly introduced as catches. The definition of the threshold which underlies the process of calculation is peculiar to the method and is as follows. In a descent the first stimulus is noted at which not all the answers to the double touches are 'two.' In an ascent the first stimulus is noted at which not more than one answer to a double touch is 'not two.' The arithmetical mean of a number of such observations is the threshold.

If larger groups are used, then in a descent the point is still noted at which the proportion of correct answers sinks to 0·8 or below 0·8; in an ascent the point at which this proportion reaches or exceeds 0·8[1]. In the experiments hereafter described that form of the method was used in which the groups consisted of five stimuli each, with five catches.

(a) *Descending Serial Groups of five.*

Let there be n stimulus values

$$r_1 < r_2 < r_3 < \dots < r_n.$$

Let the probability of an answer 'two' at each be

$$p_1 < p_2 < p_3 < \dots < p_n,$$

where $p_1 = 0$ and $p_n = 1$.

Then the probability of an answer 'not two' is at each

$$q_1 > q_2 > q_3 > \dots > q_n,$$

where $p_k + q_k = 1$.

Then the probabilities of at least one answer 'not two' occurring at any group, while none has occurred at any higher group, are

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[1] This is the pure form of the method. In some cases, however, where only one or two observations are made, it may be considered an improvement to interpolate in some simple way if the proportion 0·8 does not actually occur. This proceeding is of doubtful advantage in any case and should not be adopted if several observations can be made, for it necessitates some assumption about the psychometric function.

$$\text{II.}\qquad \begin{cases} \mathfrak{P}'_n = 1 - p^5{}_n \\ \mathfrak{P}'_{n-1} = p^5{}_n (1 - p^5{}_{n-1}) \\ \mathfrak{P}'_{n-2} = p^5{}_n p^5{}_{n-1} (1 - p^5{}_{n-2}) \\ \dots\dots\dots\dots\dots\dots\dots\dots \\ \mathfrak{P}'_k = p^5{}_n p^5{}_{n-1} \cdots p^5{}_{k+1} (1 - p^5{}_k) = \prod\limits_{\lambda=n}^{k+1} p^5{}_\lambda - \prod\limits_{\lambda=n}^{k} p^5{}_\lambda \\ \dots\dots\dots\dots\dots\dots\dots\dots \\ \mathfrak{P}'_1 = p^5{}_n p^5{}_{n-1} \cdots p^5{}_2 (1 - p^5{}_1). \end{cases}$$

The arithmetic mean of the observations will be given by

$$\text{III.}\qquad \mathfrak{T}' = \sum_{k=1}^{n} \mathfrak{P}'_k r_k.$$

It follows from the form of these expressions that this process can be applied whatever the order in which the groups are taken; and further, that the separate stimuli can be taken in any order, the process being still applicable. This is important as it enables us to apply this process to data collected by the Method of Right and Wrong Cases in order to compare it with the Method of Serial Groups.

This conclusion may be proved independently in another way. Let data be collected by the Method of Right and Wrong Cases until each stimulus value has been submitted five times, that is, let a table be filled up with n columns and five rows in any predetermined order whatsoever. Consider the five answers a, b, c, d, e, received for the value r_k of the stimulus, and take the answer a. The probability that this answer is 'not two' but that all above it in its row *are* 'two' is

$$P'_k = p_n p_{n-1} \cdots p_{k+1} q_k.$$

The probability that this answer is 'two' and that all above it in its row are also 'two' is

$$Q'_k = p_n p_{n-1} \cdots p_{k+1} p_k.$$

The probability that at least one of the answers a, b, c, d, e is 'not two,' all answers at $r_{k+1} \dots r_n$ being 'two,' is

$$\begin{aligned} &P'^5{}_k + 5P'^4{}_k Q'_k + 10P'^3{}_k Q'^2{}_k + 10P'^2{}_k Q'^3{}_k + 5P'_k Q'^4{}_k \\ &\qquad = (P'_k + Q'_k)^5 - Q'^5{}_k = Q'^5{}_{k+1} - Q'^5{}_k \\ &\qquad = Q'^5{}_{k+1} (1 - p^5{}_k) = p^5{}_n p^5{}_{n-1} \cdots p^5{}_{k+1} (1 - p^5{}_k) = \mathfrak{P}'_k, \end{aligned}$$

which is the same thing as the probability that r_k would have been noted as an observation if the data had been collected by the Method of Serial Groups. Therefore if data are collected by both these methods and this process be applied to both sets of data, any differences observed must be due to causes other than the mathematical process; and if all irrelevant causes are kept as equal as possible, then the observed differences must be due largely to the methods.

(*b*) *Ascending Serial Groups of five.*

Let there be the same series of stimuli and probabilities. Then the probability of at least four answers being 'two' at r_k is

$$p^5 + 5p^4q = p^4(5 - 4p)$$

and the probabilities of an r being noted as an observation, *i.e.* yielding at least four answers 'two' while no lesser r has done so, are

$$\text{IV.}\quad \begin{cases} \mathfrak{P}_1 = p^4_1(5 - 4p_1) \\ \mathfrak{P}_2 = \{1 - p^4_1(5 - 4p_1)\}\, p^4_2(5 - 4p_2) \\ \dots\dots\dots\dots\dots\dots\dots\dots \\ \mathfrak{P}_k = \prod\limits_{\lambda=1}^{k-1} \{1 - p^4_\lambda(5 - 4p_\lambda)\} - \prod\limits_{\lambda=1}^{k} \{1 - p^4_\lambda(5 - 4p_\lambda)\} \\ \dots\dots\dots\dots\dots\dots\dots\dots\dots\dots\dots\dots \\ \mathfrak{P}_n = \prod\limits_{\lambda=1}^{n-1} \{1 - p^4_\lambda(5 - 4p_\lambda)\} - \prod\limits_{\lambda=1}^{n} \{1 - p^4_\lambda(5 - 4p_\lambda)\}. \end{cases}$$

The arithmetical mean of the observations will be given by

$$\text{V.}\qquad \mathfrak{T} = \sum_{k=1}^{n} \mathfrak{P}_k r_k.$$

It follows again from the form of the expression for $\mathfrak{P}_k$ that the groups and the separate stimuli may be taken in any order.

Observations by the method of groups are not usually in such a form that the p's can be ascertained. The following experiments were therefore carried out to control these theoretical conclusions, by the Method of Non-Consecutive Groups.

The subject, No. 6, was a well-educated woman. The experiments were carried out with the apparatus already described but with the following improvements. Much greater care was taken to insure that the conditions of experiment were the same at each sitting. The experiments took place on fourteen days, which were consecutive save for the omission of Wednesdays, Saturdays and Sundays. The time was always between 8.45 and 9 a.m. while the college buildings were quiet. The touches were made at exactly the same intervals of time, measured by the ticks of a watch which could be heard by the experimenter but not by the subject. With his left hand, the experimenter kept the score on a set of ten counters, and at the end of each group he recorded it on the written form. The pauses between the groups were just long enough to enable this to be done and the instrument to be set for the next group. By these means 110 judgments were easily taken in ten minutes. Four days of preliminary practice were given. On each day of the actual experiments the sitting began with a group at 3 cm. which preceded the groups proper. This was done because it was found that the first group tended to be unsteady and irregular. Ten groups were then taken in the following order.

TABLE V. *Method of Non-Consecutive Groups.*

Subject, No. 6. Order of succession of the Groups.

Prelim.	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*i*	*k*	Date
3	1	4½	2½	½	1½	4	5	3½	2	3	March 11
3	4½	2½	½	1½	4	5	3½	2	3	1	,, 12
3	2½	½	1½	4	5	3½	2	3	1	4½	,, 14
3	½	1½	4	5	3½	2	3	1	4½	2½	,, 15
3	1½	4	5	3½	2	3	1	4½	2½	½	,, 19
3	4	1½	½	2½	4½	1	3	2	3½	5	,, 20
3	3	2	3½	5	1	1½	½	2½	4½	4	,, 21
3	2	3½	5	3	1½	½	2½	4½	1	4	,, 22
3	3½	5	3	1½	½	2½	4½	1	4	2	,, 25
3	5	4	1	½	2½	4½	1½	3	2	3½	,, 26

The number of answers 'two' thus obtained is shown in the following table:—

TABLE VI. *Method of Non-Consecutive Groups.*

Subject, No. 6. Number of answers 'two' at each distance.

3 cm. Prelim.	·5 cm.	1 cm.	1·5 cm.	2 cm.	2·5 cm.	3 cm.	3·5 cm.	4 cm.	4·5 cm.	5 cm.	5·5 cm.	6 cm.	6·5 cm.	Date	
—	—	—	—	—	—	—	1	2	0	5	5	—	5	March 4	Practice
—	—	—	—	—	—	—	—	—	4	—	5	—	5		
—	—	—	—	—	—	—	—	—	—	—	0	—	—		
—	—	—	—	2	4	4	5	5	5	5	5	5	5	,, 5	
—	—	1	3	1	0	4	5	5	4	5	5	—	—	,, 7	
—	—	2	1	5	4	5	1	5	5	5	5	—	—	,, 8	
1	0	*1	2	3	1	3	5	5	4	5	3½	4½	Observed Groups	,, 11	
1	3	3	3	5	4	4	5	5	*5	5	2	3		,, 12	
5	0	0	1	4	*4	4	5	5	5	5	2	3		,, 14	
0	*0	0	0	3	1	5	5	4	5	5	3	4		,, 15	
0	0	0	*0	1	0	2	3	4	5	5	4	4		,, 19	
0	0	1	1	2	5	5	5	*5	5	5	2½	2		,, 20	
5	0	1	3	0	3	*4	4	5	5	5	3	3½		,, 21	
0	0	0	2	*0	4	5	5	5	5	5	2½	2½		,, 22	
0	0	0	0	3	5	4	*3	5	5	5	2½	3½		,, 25	
0	0	0	0	3	5	5	5	5	5	*3	2½	5		,, 26	
12	3	6	12	24	32	41	45	48	49	48 = Totals					

* = the group which followed the preliminary group.

The values of $\mathfrak{P}$ and $\mathfrak{P}'$ were then calculated from the *p*'s by formulae IV and II (pp. 214, 213 respectively) and are compared in Table VII with the actual distribution of the just perceptible and just imperceptible groups[1].

TABLE VII. *Method of Non-Consecutive Groups.*

Subject, No. 6. Comparison of calculated and observed distribution. (Group Process.)

Descending (just imperceptible groups)				Ascending (just perceptible groups)			
r	p	Calculated $\mathfrak{P}'$	Actual distribution	Actual distribution	Calculated $\mathfrak{P}$	p	r
0	·028	·0000	0	0	·0000	·028	0
½	·06	·0000	0	0	·0000	·06	½
1	·12	·0000	0	0	·0009	·12	1
1½	·24	·0004	0	0	·0133	·24	1½
2	·48	·0138	1	2	·1611	·48	2
2½	·64	·1174	1	4	·3374	·64	2½
3	·82	·2233	2	2	·3788	·82	3
3½	·90	·2463	2	1	·09[illegible]6	·90	3½
4	·96	·1359	2	1	·0088	·96	4
4½	·98	·0786	1	0	·0001	·98	4½
5	·96	·1844	1	0	·0000	·96	5
		$\Sigma\mathfrak{P}'r$ $=3·67$	Arithmetical Mean = 3·5	Arithmetical Mean = 2·75	$\Sigma\mathfrak{P}r$ $=2·71$		

The words 'descending' and 'ascending' refer to the process, not the method.
The mean of 3·5 and 2·75 is 3·125.
The mean of $\Sigma\mathfrak{P}'r$ and $\Sigma\mathfrak{P}r$ is 3·19.

The agreement is sufficiently close to show that there is no gross error in the theory.

In these experiments the stimuli were presented in groups, although the groups were not consecutive. In the following experiments the stimuli were not even presented in groups, but were arranged irregularly as in the Method of Right and Wrong Cases. The data are in fact the same as are used later in this paper. No single touches were used except where the distance 0 cm. formed part of the range, and the subject knew that all the touches were really double. Unfortunately the range was not quite the same on each of the eight days on which the observations were made, and because of this the *p*'s at the extreme points, 0 cm. and 6 cm., are certainly untrustworthy; but for the present purpose let us ignore this drawback.

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[1] It must be remembered that what is here called a just perceptible group, has at least four answers 'two,' and that a just imperceptible group has at least one answer 'not two.'

TABLE VIII. *Method of Right and Wrong Cases.*

Subject, No. 4. Number of answers 'two' in ten answers.

0 cm.	1 cm.	2 cm.	3 cm.	4 cm.	5 cm.	6 cm.	
—	—	5	5½	9	8	10	
0	2	6	7	9	2/2	—	
—	1	5	6	7	9	—	
2	2	2	2	10	—	—	
—	—	4	4½	5	8	9	
—	3	4	6½	9	10	—	
2	4½	8	7	7	10	—	
0	5	6	6	9	6/6	—	
4/40	17½/60	40/80	44½/80	65/80	53/58	19/20	
·1000	·2917	·5000	·5563	·8125	·9138	·9500	$=p$

From these p's we can calculate the $\mathfrak{P}$'s and $\mathfrak{P}''$s, and form the sums $\Sigma \mathfrak{P} r$ and $\Sigma \mathfrak{P}' r$. On the other hand, from the actual data for each day's experimenting we can find two observations of the five-group threshold, by dividing the fifty judgments into two sets of twenty-five as in Table IX.

TABLE IX. *Method of Right and Wrong Cases.*

Subject, No. 4. Group Process.

2 cm.	3 cm.	4 cm	5 cm.	6 cm.	
W	R	R?	R	R	Just perceptible group = 4 cm.
W	W	R	W	R	
R?	R	R	R	R	
W	R	R	R	R	Just imperceptible group = 5 cm.
R?	W	R?	R	R	
R?	R	R?	R	R	Just perceptible group = 4 cm.
W?	?	R?	W	R	
R?	W?	R	R	R	
W	R	R?	R	R	Just imperceptible group = 5 cm.
R?	W	W	R	R	

Alike in the upper and in the lower half we get 5 cm. as the 'descending' observation and 4 cm. as the 'ascending' observation. In this way we get sixteen observations of the 'descending' kind and sixteen of the 'ascending.' The following table puts these results together.

TABLE X. *Method of Right and Wrong Cases.*

Subject, No. 4. Group Process.

Descending			Ascending		
r	Calculated $\mathfrak{P}'$	Actual distribution	Actual distribution	Calculated $\mathfrak{P}$	r
0	·0000	0	0	·0005	0
1	·0003	0	1	·0277	1
2	·0090	0	1	·1820	2
3	·1655	6	3	·2100	3
4	·3184	5	8	·4425	4
5	·2804	4	3	·1288	5
6	·2264	1	0	·0083	6
	$\Sigma\mathfrak{P}'r$ = 4·549	Arithmetical Mean = 4·000	Arithmetical Mean = 3·688	$\Sigma\mathfrak{P}r$ = 3·486	

It will be seen that considering the smallness of the sample there is again reasonable agreement between the observed arithmetical means and the calculated values. In comparing the Method of Right and Wrong Cases with the Method of Serial Groups, it has hence been shown that the same process of calculation can be applied to both, namely, the Group Process, which is usually employed with the latter method. We proceed now to consider how the probable errors of the three methods can be compared.

V. CALCULATION OF PROBABLE ERRORS.

Consider first the comparison of the Method of Right and Wrong Cases with the Method of Minimal Changes, for which the Limiting Process is to be used. Urban[1] has pointed out that there are three ways in which the probable error of an observation may be found:

1*a*. *The data are such that the probabilities P and P' can be determined, and there is only one series of stimuli.*

1*b*. *The results are not of such a kind as to permit of the evaluation of the P's and P''s, either because the number of observations is insufficient or because continually changing sets of stimuli were used.*

2. *The probabilities p are determinable.*

The data collected by the Method of Right and Wrong Cases may be dealt with under any of these heads, but the data collected by the Method of Minimal Changes can only be considered under heading 1*b*, for both of the reasons there indicated[1]. As Urban points out, it is impossible in the case in question to determine an unambiguous measure of precision. The only possible way is to make some assumption about the distribution of the observations, as, for example, happens when they are examined by the theory of least squares. The measure of precision thus obtained can only be regarded as some indication of the scatter of the observations. This, however, is the only way possible for us, and must be adopted, under the limitation expressed in the last sentence that the results are presented not as exact measures but as indications only of the scatter. In the comparison of the Method of Right and Wrong Cases with the Method of Serial Groups, the number of observations is from the nature of the process adopted, a fifth as large, and it is still less possible to judge of the distribution. These must therefore be treated in the same way; a Gaussian distribution must be assumed and the theory of least squares applied.

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[1] *Arch. f. d. ges. Psychol.* 1909, xv. 313.

To sum up, the data collected by the Method of Right and Wrong Cases can be compared with the data collected by the Method of Minimal Changes by means of the Limiting Process. The scatter of the two sets of observations in the present experiments can be compared only by making the assumptions involved in the theory of least squares.

On the other hand, the data collected by the Method of Right and Wrong Cases can be compared with the data collected by the Method of Serial Groups by means of the Group Process, a special form of the Limiting Process. Again, in the present experiments, the scatter of the observations can only be compared by the theory of least squares.

A direct comparison of the Method of Minimal Changes with the Method of Serial Groups is not possible from the data[2].

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[1] The experiments were carried out for the most part before the writer was acquainted with any of the literature on the subject, otherwise this limitation might have been partly removed by suitably planning the experiments. On the other hand the experiments would then have ceased to represent the *ordinary* form of the Method of Minimal Changes. At all events it can be claimed for the experiments here described that they were not distorted in any way to suit any mathematical process. Further experiments have been planned in which these difficulties will be faced.

[2] A direct comparison of the three methods would only be possible from data collected by the following scheme. Take a table consisting of n columns (corresponding to n stimuli

VI. COMPARISON OF THE METHODS OF RIGHT AND WRONG CASES AND MINIMAL CHANGES BY THE LIMITING PROCESS.

TABLE XI.

Totals, j. p. d.'s	r in cms.	Distribution of Lowest answer 'two' (j. p. d.) Ascending								Distribution of Highest answer 'not two' (j. i. d.) Descending								Totals, j. i. d.'s
		Subject 2		Subject 3		Subject 4		Subject 5		Subject 2		Subject 3		Subject 4		Subject 5		
		a	*b*	*a*	*b*	*a*	*b*	*a*	*b*	*a*	*b*	*a*	*b*	*a*	*b*	*a*	*b*	
22	0	0	1	3	0	4	4	5	1	2	2	2	0	6	4	4	1	24
	·5	—	1	—	0	—	2	—	1	—	1	—	0	—	1	—	1	
72	1	11	1	15	3	17	3	9	2	8	0	6	2	14	3	8	5	56
	1·5	—	1	—	4	—	1	—	5	—	2	—	1	—	4	—	3	
90	2	15	3	10	4	27	2	9	4	8	2	11	4	17	4	9	3	80
	2·5	—	3	—	4	—	8	—	1	—	7	—	6	—	5	—	4	
64	3	9	2	14	3	14	3	10	3	18	5	18	3	25	6	13	2	102
	3·5	—	1	—	2	—	2	—	1	—	4	—	5	—	1	—	2	
70	4	10	6	8	5	14	3	12	4	11	4	18	6	12	2	9	2	73
	4·5	—	3	—	2	—	2	—	1	—	3	—	2	—	1	—	3	
32	5	4	3	9	3	2	1	7	1	3	0	5	1	5	0	10	1	27
	5·5	—	2	—	0	—	—	—	1	—	2	—	0	—	—	—	0	
13	6	1	2	1	0	2	—	6	—	0	1	0	1	1	—	6	—	9
	6·5	—	1	—	—	—	—	—	—	—	0	—	—	—	—	—	—	
1	7	—	—	—	—	—	—	1	—	—	—	—	—	—	—	0	—	0
	n =	50	30	60	30	80	31	59	25	50	33	60	31	80	31	59	27	

a = Method of Right and Wrong Cases.
b = Method of Minimal Changes.

j. p. d. = Just Perceptible Difference.
j. i. d. = Just Imperceptible Difference.

TABLE XII.

Arithmetical Means and Probable Errors of the Observations.

Limiting Process.

Subject	Method of Right and Wrong Cases				Method of Minimal Changes				
	Thresh-old	P. E. of mean	P. E. of one	°/₀ P. E. of one	°/₀ P. E. of one	P. E. of one	P. E. of mean	Thresh-old	
2	2·680	0·1285	0·9085	33·90	31·55	1·136	0·2073	3·600	Lowest answer 'two' (ascending) j. p. d.
3	2·667	0·1357	1·052	39·42	29·83	0·8651	0·1580	2·900	
4	2·387	0·1007	0·9011	37·74	42·66	0·9840	0·1767	2·307	
5	3·100	0·1629	1·251	32·05	38·29	0·9802	0·1960	2·560	
2	2·740	0·1200	0·8488	30·98	31·75	0·9668	0·1683	3·045	Highest answer 'not two' (descending) j. i. d.
3	2·983	0·1081	0·8381	28·09	24·86	0·7780	0·1397	3·129	
4	2·525	0·1039	0·9294	36·82	40·39	0·8343	0·1498	2·065	
5	3·170	0·1539	1·182	37·30	38·31	0·9295	0·1823	2·426	

The probable error of the mean is calculated from the formula

$$0{\cdot}6745\sqrt{\frac{\Sigma v^2}{n(n-1)}},$$

that of one observation from the formula

$$0{\cdot}6745\sqrt{\frac{\Sigma v^2}{n-1}},$$

where the v's are the residuals and n is the number of observations, given in the bottom row of Table XI. The percentage P.E. of one observation expresses the P.E. of one observation as a percentage of the corresponding threshold.

in order), and of 5 or 10 rows. Fill this table up by presenting the stimuli in three different ways:

(1) Row by Row, beginning (*a*) always at the left,
(*b*) always at the right;
(2) Column by Column, beginning (*a*) with the left-hand Column,
(*b*) with the right-hand Column;
(3) In any irregular but predetermined order.

In (2) catch cases could be introduced. When many such tables had been filled in, one chosen mathematical process could be applied to each group, and an accurate comparison both of threshold and scatter obtained. The third method is that of Right and Wrong Cases, the second that of Serial Groups. The first method, however, is not the ordinary form of the Method of Minimal Changes, because in the ordinary form it is customary to begin at different points and to vary the steps.

Some interesting observations may be deduced from this table.

(i) In the Method of Right and Wrong Cases the just perceptible difference is in every case slightly less than the just imperceptible difference. In the Method of Minimal Changes, however, the reverse statement is true in three cases.

(ii) The thresholds for the 'uninformed' subjects 2 and 3 are lower by the Method of Right and Wrong Cases than by the Method of Minimal Changes. For the two 'informed' subjects 4 and 5 the reverse statement is true.

(iii) The P.E. of one observation is in seven of the eight cases greater for the just perceptible difference than for the just imperceptible. If we take the *percentage* P.E. of one, however, the proportion is reduced to five of the eight cases.

(iv) The P.E. of one observation is greater by the Method of Right and Wrong Cases in five of the eight cases. If we take the *percentage* P.E. of one, however, this proportion is reduced to three in eight. These three cases occur among the 'uninformed' subjects.

It is evident therefore that no definite conclusion can be drawn from the experiments. More subjects are needed, and more experiments on each subject. This becomes still more evident if a more careful examination be made of any one of the above four observations. The most precise is, (ii) that for the 'uninformed' subjects the 'Minimal Changes' threshold is higher, for the 'informed' subjects the 'Right and Wrong' threshold is higher. The proper way to test the reliability of this conclusion is to determine the difference in each case and also to calculate the probable error of the difference. This is done in Table XIII, where the contrast between 'informed' and

TABLE XIII. *Difference between the Thresholds found by the Methods of Right and Wrong Cases and Minimal Changes.*

	Subject	Lowest answer 'two' (ascending) j. p. d.		Highest answer 'not two' (descending) j. i. d.	
		Difference	P. E. of differ.	Difference	P. E. of differ.
uninformed	2	0·920	0·244	0·305	0·207
	3	0·233	0·208	0·146	0·177
informed	4	-0·080	0·203	-0·460	0·182
	5	-0·540	0·255	-0·744	0·239

The difference is reckoned positive when the 'Minimal Changes' limen exceeds the 'Right and Wrong Cases' limen.

'uninformed' subjects appears in the difference in sign, while the size of the probable errors shows that there is little or no reason to think that this may not be due to chance. At the most it is a straw to show how the wind is perhaps blowing.

TABLE XIV. *Mean percentage P. E. of one observation.*

	Method of Right and Wrong Cases	Method of Minimal Changes
Ascending (j. p. d.)	35·78	35·58
Descending (j. i. d.)......	33·30	33·83

Table XIV shows the mean percentage P.E. of an observation by each method, and for just perceptible and just imperceptible differences. Again, of course, no emphasis may be laid on the numbers, for they are subject to huge probable errors. But the fact that j. p. d.'s are more scattered than j. i. d.'s agrees with the general impression of the experimenter and with several other points to be noted later. Compare also the extreme right and left columns of Table XI giving the totals for all four subjects, when it is seen that the distribution of the j. i. d.'s is much more regular than that of the j. p. d.'s.

VII. COMPARISON OF THE METHODS OF RIGHT AND WRONG CASES AND SERIAL GROUPS BY THE GROUP PROCESS.

TABLE XV.

r in cms.	Distribution of just perceptible groups (ascending)								Distribution of just imperceptible groups (descending)							
	Subject 2		Subject 3		Subject 4		Subject 5		Subject 2		Subject 3		Subject 4		Subject 5	
	a	*c*	*a*	*c*	*a*	*c*	*a*	*c*	*a*	*c*	*a*	*c*	*a*	*c*	*a*	*c*
0	—	—	—	0	0	—	0	—	—	—	—	—	—	—	—	—
1	—	0	1	0	1	0	0	—	—	—	—	—	—	—	1	—
2	2	0	0	0	1	2	3	0	—	2	—	1	—	—	0	—
3	2	3	2	1	3	1	0	0	2	1	—	0	6	1	2	1
4	3	2	4	4	8	1	5	1	5	0	8	2	5	2	4	0
5	3	0	4	2	3	1	0	0	3	0	4	0	4	0	2	1
6	—	2	1	1	—	—	2	1	—	1	0	2	1	—	3	0
7	—	—	—	—	—	—	2	—	—	—	—	—	—	—	0	0
$n=$	10	7	12	8	16	5	12	2	10	4	12	5	16	3	12	2

a = Method of Right and Wrong Cases.
c = Method of Serial Groups.

TABLE XVI.

Arithmetical Means and Probable Errors of the Observations.

Group Process.

Subject	Method of Right and Wrong Cases				Method of Serial Groups				
	Threshold	P. E. of mean	P. E. of one	% P. E. of one	% P. E. of one	P. E. of one	P. E. of mean	Threshold	
2	3·700	0·2473	0·7820	20·90	21·91	0·9074	0·3430	4·143	j. p. group ascending
3	4·083	0·2553	0·8843	21·66	13·96	0·6180	0·2185	4·375	
4	3·688	0·2152	0·8611	23·34	27·49	0·8794	0·3932	3·200	
5	4·333	0·3555	1·231	28·42	19·07	0·9539	0·6745	5·000	
2	4·100	0·1573	0·4976	12·13	39·28	1·276	0·6384	3·250	j. i. group descending
3	4·333	0·0959	0·3322	7·67	25·65	1·129	0·5048	4·400	
4	4·000	0·1629	0·6516	16·29	10·62	0·3894	0·2248	3·667	
5	4·250	0·2892	1·002	23·57	23·84	0·9539	0·6745	4·000	

TABLE XVII. *Mean Probable Errors of Methods of Right and Wrong Cases and Serial Groups.*

Group Process.

Method of Right and Wrong Cases			Method of Serial Groups			
Threshold	P. E. of one	% P. E. of one	% P. E. of one	P. E. of one	Threshold	
3·952	0·9396	23·58	20·61	0·8397	4·179	ascending, j. p. g.
4·171	0·6209	14·92	24·85	0·9371	3·829	descending, j. i. g.

It cannot be said that this comparison shows any important difference between the methods in question. In the Method of Right and Wrong Cases the scatter of the just perceptible groups is greater than that of the just imperceptible groups, corresponding to an observation made in using the Limiting Process; but this phenomenon does not occur in the Method of Serial Groups.

VIII. COMPARISON OF THE LIMITING AND GROUP PROCESSES, BASED ON DATA COLLECTED BY THE METHOD OF RIGHT AND WRONG CASES.

TABLE XVIII.

Subject	Limiting Process					Group Process				
	Thresh-old	P. E. Mean	P. E. one	P. E. five	°/₀ P. E. five	°/₀ P. E. one	P. E. one	P. E. Mean	Thresh-old	
2	2·680	0·1285	0·9085	0·4062	15·16	20·90	0·7820	0·2473	3·700	ascending
3	2·667	0·1357	1·052	0·4704	17·64	21·66	0·8843	0·2553	4·083	
4	2·387	0·1007	0·9011	0·4029	16·88	23·34	0·8611	0·2152	3·688	
5	3·100	0·1629	1·251	0·5594	18·04	28·42	1·231	0·3555	4·333	
2	2·740	0·1200	0·8488	0·3796	14·04	12·13	0·4976	0·1573	4·100	descending
3	2·983	0·1081	0·8381	0·3748	12·54	7·67	0·3322	0·0959	4·333	
4	2·525	0·1039	0·9294	0·4157	16·46	16·29	0·6516	0·1629	4·000	
5	3·170	0·1539	1·182	0·5286	16·67	23·57	1·002	0·2892	4·250	

P. E. five means P. E. of mean of five, *i.e.* $\frac{\text{P. E. of one}}{\sqrt{5}}$.

TABLE XIX. *Mean Probable Errors for Subjects* 2, 3, 4, *and* 5.

Limiting Process		Group Process		
P. E. five	°/₀ P. E. five	°/₀ P. E. one	P. E. one	
0·4597	16·93	23·58	0·9896	ascending
0·4247	14·93	14·91	0·6208	descending

Here the two *processes* are applied to the same data. It seems fair to compare the P.E. of *one* in the Group Process with the P.E. of the mean of *five* in the Limiting Process. The Limiting Process has if anything the best of the comparison. In both processes the 'just perceptibles' are more scattered than the 'just imperceptibles.'

IX. COMPARISON OF THE LIMITING AND THE CONSTANT PROCESSES.

These are the two types of calculating process which are at present in the field—the Group Process being a special form of the Limiting Process. The assumption of some special form of the psychometric function is frequent in the Constant Process but might also be made in the Limiting Process. In fact, this has been done earlier in this article when the theory of least squares was used in calculating probable errors.

The essential difference between the two methods lies in the way the distribution of the limina is represented. Compare first the Constant Process with the descending Limiting Process. In the former the proportion of limina ***between*** r_k and r_{k-1} is

$$\text{VI.} \qquad \Delta p_{k,\,k-1} = p_k - p_{k-1}.$$

In the latter the proportion of limina (j. i. d.'s) *at* r_k is

$$\text{VII.} \qquad P'_k = \prod_{\lambda=n}^{k+1} p_\lambda - \prod_{\lambda=n}^{k} p_\lambda.$$

The descending Limiting Process therefore replaces the p's by the continued product of the p's from a point where $p = 1$, and gives them a shift upward. In the ascending Limiting Process,

$$\text{VIII.} \qquad P_k = \prod_{\lambda=1}^{k-1} (1 - p_\lambda) - \prod_{\lambda=1}^{k} (1 - p_\lambda),$$

so that the distribution of the j. p. d.'s depends on a very similar function, a continued product of $(1 - p)$ or q from $p = 0$, which descends as r ascends.

This contrast of the two methods is well seen in a comparison of Urban's formula

$$T = \Sigma P r$$

with Spearman's formula[1]

$$L = \frac{\Sigma c p}{100},$$

in which $\frac{p}{100}$ is Δp in our notation and c is some point between consecutive r's.

It is instructive to compare the distribution, in an actual case, of these differently defined limina. This can be done with the data of the experiments on subjects 2, 3, 4, 5 by the Method of Right and

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[1] This *Journal*, 1908, II. 234.

Wrong Cases. Table XX gives the values of the p's for each subject, that is the proportion of answers 'two' at each distance. From these the Δp's can be formed and are given in Table XXI. The columns headed $\bar{P}$ in this table show the actual observed distribution of the j. p. d.'s and j. i. d.'s put together. The $\bar{P}$'s form in each case a regular series, rising to a maximum and then falling; whereas the Δp's are very irregular and sometimes even negative, which is unthinkable.

TABLE XX. *Raw values of p.*

r cms.	Subject 2	Subject 3	Subject 4	Subject 5
0	—	* ·4286	* ·1000	* ·5555
1	·2390	·2833	·2917	·3965
2	·4000	·2833	·5000	·5278
3	·4400	·5083	·5563	·4407
4	·7400	·6417	·8125	·6610
5	·9375	·9151	·9138	·7797
6	—	—	* ·9500	* ·8000
7	—	—	—	* 1·0000

* These values are untrustworthy, being deduced from fewer observations, which, moreover, were made on days when the threshold was noticeably higher or lower than usual.

TABLE XXI. *Comparison of* Δp *and* $\bar{P}$ (observed).

r cms.	Subject 1		Subject 2		Subject 3		Subject 4	
	Δp	$\bar{P}$	Δp	$\bar{P}$	Δp	$\bar{P}$	Δp	$\bar{P}$
			·4286		·1000		·5555	
0	—	·020	—	·042	—	·063	—	·009
	·2390		- ·1453		·1917		- ·1590	
1	—	·190	—	·175	—	·193	—	·101
	·1610		·0000		·2083		·1313	
2	—	·230	—	·175	—	·275	—	·144
	·0400		·2250		·0563		- ·0871	
3	—	·270	—	·267	--	·244	—	·178
	·3000		·1334		·2562		·2203	
4	—	·210	—	·217	—	·162	—	·195
	·1975		·2734		·1013		·1187	
5	—	·070	—	·117	—	·044	—	·153
	·0625		·0849		·0362		·0203	
6	—	·010	—	·008	—	·019	—	·144
					·0500		·2000	
7	—		—		—		—	·076

The values of Δp represent limina *between* the stimulus values, the values of $\bar{P}$ represent limina *at* those values.

One advantage of the *P*'s is that whether observed (as here) or calculated from the *p*'s, a negative value is impossible. But a negative value ought to be impossible by the Constant Process too. Δp measures the number of limina in the region Δr only when the judgments at r and $r + \Delta r$ have been very numerous. In other circumstances the number of limina in this region is better represented by another expression which must become identical with Δp when the number of judgments becomes very great. This expression is deduced in the next paragraph.

X. AN IMPROVED FORM OF TOTALLING IN THE CONSTANT PROCESS.

It is assumed as implied in the word 'limen' that an answer 'two' for a distance of r cms. means that at that moment the limen is below r cms.: an answer 'one' means that the limen at that moment is above r cms.

Take a very simple and extreme case of two stimulus values and a reversal of the first order. Let ten judgments be made at 2 cms. and ten at 3 cms. and let the answers be as follows:

$r=$	2 cms.	3 cms.
Two	6	3
One	4	7

The threshold has been examined on twenty occasions. On four of these it was above 2 cms. and on seven *other* occasions it was above *both* 2 cms. and 3 cms. On three occasions it was below 3 cms. and on six other occasions it was below both 2 cms. and 3 cms. We have, therefore, (if we neglect the weights of these observations)—

$r=$	2 cms.	3 cms.
No. of occasions on which limen $<r$...	6	6+3
No. of occasions on which limen $>r$...	7+4	7
Totals...	17	16

On seventeen of the twenty occasions therefore the answer told something of the position of the limen relative to 2 cms. and on six of these seventeen it was below 2 cms. As far as our information goes therefore the probability that the limen will on any occasion be below 2 cms. is

$$p'_2 = \tfrac{6}{17} = \cdot 3529.$$

Similarly $$p'_3 = \tfrac{9}{16} = \cdot 5625.$$

The probability of the limen lying between 2 cms. and 3 cms. is

$$\Delta p'_{2,3} = \cdot 5625 - \cdot 3529 = \cdot 2096.$$

By this procedure therefore every answer 'two' will be counted not only at the distance at which it is given but *also at every larger distance*. Every answer 'one' will be counted not only at the distance at which it is given but also at every smaller distance. An answer 'undecided' will be counted as ½ at the distance at which it is given, but as a wrong answer below that distance and as a right answer above it; and then—

IX. $$\Delta p'_{k,\,k-1} = p'_k - p'_{k-1}.$$

By this procedure reversals of the first order become impossible.

TABLE XXII. *Method of Right and Wrong Cases.*

Subject, No. 4.

cms....	0	1	2	3	4	5	6
Limen above	48 40 37 15 5 1	42½ 40 37 15 5 1	40 37 15 5 1	35½ 15 5 1	15 5 1	5 1	1
Limen below	4	4 17½	4 18 40	4 18 40 44½	4 18 40 46 65	4 18 40 46 65 53	4 18 40 46 65 53 19
Above...	141	140½	98	56½	21	6	1
Below...	4	21½	62	106½	173	226	245
Totals...	145	162	160	163	194	232	246
p'......	·028	·133	·388	·653	·892	·974	·996

$\Delta p'$... ·028 ·105 ·255 ·265 ·239 ·082 ·022 ·004

This Table is formed from the fractions 4/40, etc. in Table VIII.

Table XXII shows this mode of totalling carried out for subject No. 4.

Apart from theoretical considerations, when this procedure is carried out for the Method of Right and Wrong Cases for subjects 2, 3, 4 and 5, values of p' are obtained which are given in Table XXIII, and from these the values of $\Delta p'$ are calculated which are in Table XXIV compared with the values of $\bar{P}$. In practically every case the values of $\Delta p'$ fit into place between the proper values of $\bar{P}$. In these cases then this procedure brings the Constant and Limiting Methods into close agreement.

TABLE XXIII. *Values of* p'.

r in cms.	Subject 2	Subject 3	Subject 4	Subject 5
0	·018	·020	·028	·047
1	·099	·122	·133	·143
2	·308	·270	·388	·313
3	·626	·554	·653	·500
4	·853	·804	·892	·722
5	·978	·975	·974	·885
6	1·000	1·000	·996	·966
7	—	—	1·000	1·000

TABLE XXIV. *Comparison of* $\Delta p'$ *and* $\bar{P}$ (observed).

r cms.	Subject 2		Subject 3		Subject 4		Subject 5	
	$\Delta p'$	$\bar{P}$	$\Delta p'$	P	$\Delta p'$	P	$\Delta p'$	$\bar{P}$
	·018		·020		·028		·047	
0	—	·020	—	·042	—	·063	—	·009
	·081		·102		·105		·096	
1	—	·190	—	·175	—	·193	—	·101
	·209		·148		·255		·170	
2	—	·230	—	·175	—	·275	—	·144
	·318		·284		·265		·187	
3	—	·270	—	·267	—	·244	—	·178
	·227		·250		·239		·222	
4	—	·210	—	·217	—	·162	—	·195
	·125		·171		·082		·163	
5	—	·070	—	·117	—	·044	—	·153
	·022		·025		·022		·081	
6	—	·010	—	·008	—	·019	—	·144
					·004		·034	
7	—	—	—	—	—	—	—	·076

The values of $\Delta p'$ represent limina *between* the stimulus values, the values of $\bar{P}$ represent limina *at* those values.

XI. THE PSYCHOMETRIC FUNCTION.

If this procedure is applied to the case where an infinite number of values of r are examined and an infinite number of judgments made at each, it appears to lead to a mathematical expression for the psychometric function.

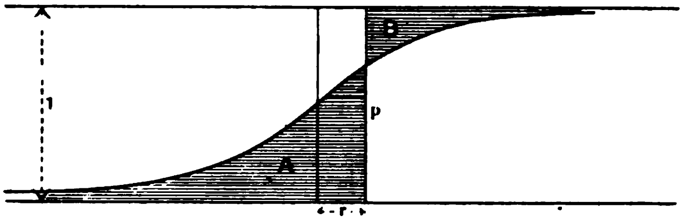

FIG. 1.

Let the curve in the figure show the variation of p with r and let r be measured from the point where $p = \frac{1}{2}$. Then the shaded area A represents the answers 'two' below r and the shaded area B represents the answers 'one' above r and

X. $$p' = \frac{A}{A+B},$$

which measures the probability that the threshold will at any moment be below r. But p also measures this probability directly, and if an infinite number of judgments have been taken we must have

$$p = p',$$

that is XI. $$p = \frac{A}{A+B}.$$

This relation defines families of curves obtained by the solution of the corresponding differential equation. With the limitation that the curve is symmetrical with respect to the point $(0, \frac{1}{2})$ we have[1]

$$A + B = \frac{A}{p} \quad \text{.................................(i)},$$

$$A - B = r \quad \text{................................(ii)};$$

$$\therefore\ A = \frac{rp}{2p-1}. \quad \text{But } \frac{dA}{dr} = p.$$

$$\therefore\ p = \frac{d}{dr}\frac{rp}{2p-1}, \text{ which gives } \frac{dp}{dr} = \frac{2p(1-p)(2p-1)}{r}.$$

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[1] I have to thank Mr T. H. Havelock for this neat solution.

The solution is

$$\text{XII.} \qquad h^2r^2 = \frac{(2p-1)^2}{p(1-p)}, \text{ or } hr = \frac{p-q}{\sqrt{pq}},$$

where h is an arbitrary parameter, a measure of precision. With this equation the relation (1) can be verified by direct integration. A graph of this function is given in figure 2. It has two branches corresponding to p and q.

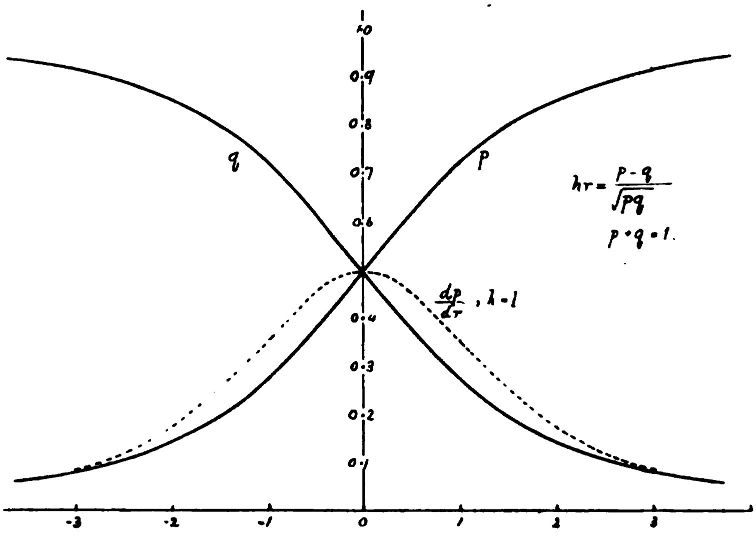

FIG. 2.

The writer is not unaware of the difficulty to which the above result leads. It would appear to be an *a priori* proof of the form of the psychometric function based on no experimental evidence; and this is impossible. There is either some fallacy in the reasoning (which the writer cannot discover), or the assumptions upon which the reasoning is based contain more than is at first apparent. The explicit assumptions have been carefully given; there may however at some point be implicit assumptions which have escaped notice.

Here it may be remarked that the question of the precision of the different p's has not been entered upon. It is known that, *ceteris paribus*, a p near the value ·5 is less precise than one near the values 0 or 1, and this precision is proportional to

$$\sqrt{\frac{1}{p(1-p)}}.$$

It is curious that this expression should form part of the new function. There may be some deeper connexion with Bernoulli and Poisson's theory of precision, and it may be the tacit assumption that all the p's are equal in precision which has brought the above expression into the function.

Whatever be thought of its theoretical deduction, the formula XII may be put forward as yet another possible expression for the psychometric function and may perhaps be found to fit better than those already tried. The relation XI includes the conditions which such a function is usually required to fulfil.

XII. CHANGES IN THE THRESHOLD DURING A SITTING.

The threshold is always in motion, oscillating about some average value. This average value is itself probably in slow motion; it can be lowered with practice and (so it has been asserted) it increases with fatigue. It is of interest to try to follow the movements of the threshold during a sitting, to seek whether any law governs its oscillations and whether anything can be predicted of the changes in the average value during a sitting, that is to say, whether practice and fatigue during one sitting have any noticeable effect. In an attempt to attack this question the following graphic method of recording judgments was employed.

The judgments, which were made at approximately equal intervals of time, were set out on squared paper with times as abscissae and centimetres as ordinates, answers 'two' being marked by rings and answers 'one' by crosses (see figure 3). The continuous line separating all the rings from the crosses is drawn as nearly horizontal as possible.

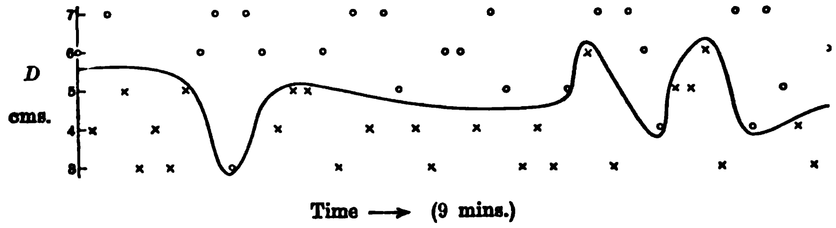

FIG. 3.

In a sense it represents the rise and fall of the threshold during the nine minutes over which the experiment extended. An average value of the threshold would be found by obtaining the average height of this line above the x axis, an operation which could be performed with a planimeter. The average height of the crests above this, or the average depth of the troughs below, measures the probable error of one observation.

The limitations of the device and the assumptions it makes must be pointed out. In the first place the diagram represents the threshold by a continuous line, whereas it is only measured at intervals, and nothing is known of it between these points. Secondly, the continuous line separating rings from crosses can clearly be moved about very considerably. This would not perhaps appreciably change its general form but would cause great changes in its average height (corresponding to error of the mean). It is however a distinct advantage that the vagueness of threshold measurements should be so clearly seen.

Such graphic records were made of all the experiments by the Methods of Right and Wrong Cases and Minimal Changes on subjects 2, 3, 4 and 5, and although the experiments are not sufficiently extended to permit any generalisation to be made, yet they suggest two possibilities:—(1) that there may be some rhythm in the rise and fall of the limen itself; (2) that its mean value seems to sink up to about fifty judgments (presumably with practice) and then to rise again (presumably with fatigue or boredom).

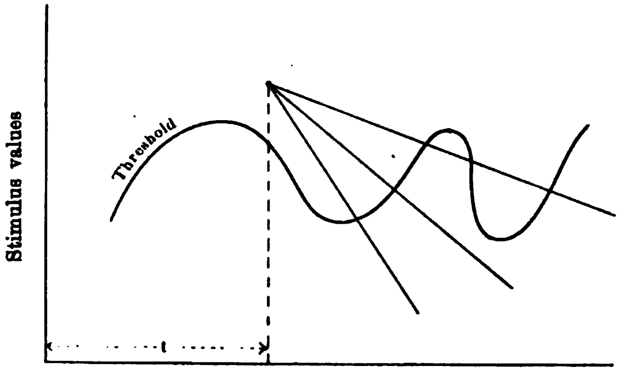

FIG. 4.

It may be remarked, with regard to the first point, that damped oscillations, frequently restarted, would give a distribution of the limen not unlike those found. Also the rate of succession of the stimuli would in this case be important. This rate of succession (in time) of the stimuli has of course probably a psychological effect on the subject, but apart from this, and assuming even that the oscillations of the limen are quite independent thereof, a rapid descending succession of stimuli would give a different result from the same succession of stimuli presented at longer intervals of time. This is made clear by the figure 4, in which, starting from the time *t*, a rapid descent would give a low threshold, a slower descent a higher threshold, and a very slow descent a reversal.

Observation of figure 3 shows that the continuous line could have been placed with much more certainty had the judgments been more judiciously spaced. Especially is this the case in the neighbourhood of the first depression. This suggests that a standard order of succession for stimuli would be an advantage in making observations by different observers more comparable, both for the reason just indicated and also because the psychological effects of expectation etc. would be more certainly the same in different experiments. The following order is suggested for five stimulus values, giving fifty judgments in all. For children the first twenty-five would be sufficient for one sitting, the second twenty-five being given at a second sitting. For young children further subdivision could be made.

TABLE XXV.

Stimulus values......	r_1	r_2	r_3	r_4	r_5
Order of succession ...	*a*	*c*	*e*	*b*	*d*
	c	*e*	*b*	*d*	*a*
	e	*b*	*d*	*a*	*c*
	b	*d*	*a*	*c*	*e*
	d	*a*	*c*	*e*	*b*
	b	*e*	*c*	*a*	*d*
	e	*c*	*a*	*d*	*b*
	c	*a*	*d*	*b*	*e*
	a	*d*	*b*	*e*	*c*
	d	*b*	*e*	*c*	*a*

The second point mentioned, that the mean value of the limen appears during one sitting at first to sink and then to rise again was examined statistically as follows.

The data collected both by the Method of Right and Wrong Cases and Minimal Changes were employed, and for each subject the limen and its P.E. were deduced by the Limiting Process from data made up of

(*a*) the first twenty-five judgments at each sitting,

(*b*) the second twenty-five judgments at each sitting, and so on. The results were not unfavourable to the supposition of a fall followed by a rise, but the probable errors of the means are very large, and in most of the cases the probable error of the difference observed was so large as to discredit any inference. In only one case did the difference exceed 4½ times its P.E.; the numbers in this case are as follows:

TABLE XXVI.

Subject, No. 2.

	Threshold	P. E. of one	P. E. of mean
Deduced from first 25 judgments in each sitting	3·709	1·068	0·1525
,, ,, second ,, ,, ,, ,,	2·714	1·017	0·1453
Difference.....................	0·995		
P. E. of difference	0·221		

In this case therefore it is fairly certain that the mean value of the limen fell during a sitting, up to about 50 judgments. The data given earlier on page 215 show that this was the case also with subject No. 6. The experiments on this subject were conducted by the Method of Non-Consecutive Groups, each sitting beginning with a group of five judgments at 3 cms. with five catches. This group was preliminary to the real sitting in which the group 3 cms. again occurred.

TABLE XXVII.

Subject, No. 6.

	Preliminary groups at 3 cms.	Later groups at 3 cms.
Total judgments.........	50	50
Answers 'two'............	12	41

It will be seen that the preliminary groups indicate a much higher limen than the others. Additional evidence is gained from an inspection of Table VI, page 215, where the first of the regular groups often gives a low number of *two*'s: note especially the only occasion when answers *one* were returned at 5 cms.

In the case of subjects 3, 4 and 5 a similar phenomenon was suspected but could be neither proved nor disproved from the data. With all the subjects it was suspected that after about fifty judgments the limen rose again with fatigue (or boredom), but this again could not be proved or disproved.

It may be that fatigue and practice during one sitting may partly explain the contradictory results obtained by those who have investigated the spatial threshold as a measure of fatigue. Griesbach deduced the value of the threshold very quickly after a very short sitting and his results may be expected to differ from those obtained in a longer sitting. The probable error of these rapidly obtained results however must be enormous. It is impossible to believe that they can serve as a measure of anything.

XIII. INTROSPECTION.

The subjects were encouraged to report their introspection in the course of these experiments as freely as possible and were occasionally asked immediately after a sitting to write down anything they believed would be of interest in connexion with the investigation. No leading questions were put although one or two points were suggested for examination, as *fatigue*, and (in the case of the 'informed' subjects) the *influence of the method employed.*

Variation of the Threshold.

Two of the subjects detected a rapid oscillation of the threshold, especially at distances of the compass points roughly corresponding to the average threshold.

No. 3. "Sometimes the first sensation is *two*, later it becomes *one*. I say *one* when this happens. Often there is a wavering impression, one point remaining steady while the pressure at the other appears to increase and decrease." "It sometimes seems as if the upper one were put on, and the lower one slowly appears, grows up. Sometimes the upper one appears firm and the lower one appears as if tapping. Sometimes, but rarely, it is the lower one that is firm."

No. 4. "When the distance apart of the points has reached a certain magnitude, each point gives a clear and definite sensation. But at distances which appear a little greater than this, a peculiar and persistent wavering, pulsating, or oscillating between blunt 'one' and sharp 'two' sets in."

It is of course possible that on some occasions actual oscillation of the aesthesiometer occurred, but the writer's belief is that these were subjective impressions.

No. 4 gave very great attention to the sensations and rarely gave an answer without qualifying it in some way, not only by expressions of greater or less doubt but by descriptive words such as 'painful,' 'peculiarly sharp,' 'a central touch and a diffuse area round about.' On one occasion when his threshold was noticeably more definite than usual, these remarks were absent and he explained later that he had found it impossible to give his attention to the experiment as he had recently lost a relative in an accident and was to attend the funeral that day. Again, the boy No. 1, who did not try at all to give great attention, had a much more definite threshold than the other subjects.

Fatigue and practice during the sitting.

No. 2. "When two successive sets were done on the same day, the decisions in the second set seemed on the whole quicker and easier to make than in the first."

No. 5. "I feel I am getting erratic and not to be depended upon though I am not tired." "I have an idea that I gave more accurate judgments towards the end, especially after I had experienced the pressure of two definite points."

This subject also complained of his arm tingling, and of the presence of 'reminiscences of previous touches.' No. 4 complained on one occasion of a 'deadened' feeling towards the end of the sitting and on another occasion remarked "I don't think my arm is so sensitive after the first fifty. There seems to be no definition about them."

Eyes. No. 2 discovered that it was an assistance if he directed his eyes towards the part of his arm which was being tested. Nos. 3, 5, and 6 each announced that they found it easier with closed eyes.

Quietness in room and neighbourhood. All objected to any disturbance however slight. No. 2 said "It is surprising how difficult it is to decide when anyone is talking in the neighbourhood."

No. 6. "You cannot hear and feel at the same time."

Feeling of pain. Several subjects spoke of the feeling of pain occurring at some touches independently of the pressure.

No. 2. "Sometimes all distinction between double and single touch is overwhelmed by a feeling of pain."

No. 4. "I can feel one point *so much* that I cannot tell if there is another."

Blunt feeling of double touch below the threshold. All the subjects remarked upon this. They were instructed to answer 'one' unless two separate points could be felt. No. 5 pointed out that he found it difficult to decide when this occurred. The bluntness was frequently compared to the butt end of a pencil. Nos. 4 and 5 (independently of one another) drew a small circle and a larger oval to represent single touch and double-touch-below-the-threshold respectively. Several subjects declared that they could distinguish degrees of bluntness below the threshold.

Expectation. No. 5, who was 'fully informed,' said that in an ascending series this feeling of increasing bluntness warned him that a double touch would follow. This subject considered that it was more difficult to avoid letting his information bias his judgments in the Method of Minimal Changes than in any other method. No. 2 said "If two or three similar sensations (i.e. two or three 'ones' or two or three 'twos') follow each other, decision seems to grow easier each time although there was no appreciable difference in the sensation." Evidently No. 2 tends to repeat the last judgment on each occasion: and it is noticeable that the evidence of difference between ascents and descents is rather stronger for this subject than for any other. The figures are given in Table XXVIII.

TABLE XXVIII.

Subject, No. 2.

	Minimal Changes. Limiting Process	Serial Groups. Group Process
Ascending	3·600	4·143
Descending	3·045	3·250
Difference	0·555	0·893
P. E. of difference	0·267	0·725

The differences, small though they are, are in a direction consistent with the introspection of the subject who, it should be remembered, was quite 'uninformed.'

XIV. SUMMARY.

In measurements of any threshold it is important that a clear distinction be drawn between methods of experimenting and processes of calculating. The names here used for methods of experimenting are as follows: The Method of (*a*) Minimal Changes, (*b*) Right and Wrong Cases, (*c*) Serial Groups, and (*d*) Non-Consecutive Groups. The last is a new method. The names used for processes of calculation are (*a*) The Limiting, (*b*) The Constant, and (*c*) The Group Processes. These are usually but not necessarily applied to the similarly lettered methods. The theory of the Group Process has been developed sufficiently to show that it can be applied to the Method of Right and Wrong Cases to which Urban has previously shown that the Limiting Process can also be applied.

Measurements of the spatial threshold have been made, by all the above methods, on six subjects. The results are far from conclusive but suggest a difference between informed and uninformed subjects. The writer, as a result of the experience gained, prefers the Group Methods but considers that the Group Process needs modification in the direction of lowering the arbitrarily chosen eighty per cent.[1] The Group Process was compared with the Limiting Process by using both on the same data. The former gives of course a higher threshold, and although the P.E. of one Group-observation is less than that of one Limiting-observation, it is greater than the P.E. of the mean of five Limiting-observations. Just perceptibles seem to be more scattered than just imperceptibles.

The Constant Process measures the frequency of its individual thresholds, in a region Δr, by Δp; the Limiting Process actually notes the number of its individual thresholds at each stimulus value. When the data are scanty Δp is found to vary irregularly and even to become negative, whereas the Limiting Process with the same data gives a more regular distribution. A new procedure for totalling in the Constant Process is therefore suggested, which makes reversals of the first order impossible. It is shown that this procedure brings the Constant Process into close agreement with the Limiting Process in the case of all the subjects examined. The same idea leads to the suggestion of a new form of the psychometric function given by

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[1] If fifty per cent. be not chosen, it may be pointed out that taking seventy-five per cent. for descending Serial Groups and twenty-five per cent. for ascending Serial Groups would give immediately an approximate measure of scatter.

$$hr = \frac{p-q}{\sqrt{pq}}.$$

The experiments permit an examination to be made of changes in the threshold during one sitting. A graphic method of recording was employed for this purpose and the data were also examined statistically. In two cases it was shown that the average threshold fell during the earlier part of a sitting. This was also suspected in the other subjects and in all cases it was suspected that later in the sitting a rise occurred. The possibility of a rhythmic rise and fall every few seconds was considered and the influence of the rate of succession of the stimuli pointed out. The introspective details given by the subjects tend to confirm the results of the calculations. Evidently individual differences are very important. Very close attention by the subject appears to increase the variation of the threshold.

(*Manuscript received April* 4, 1912.)

PUBLICATIONS RECENTLY RECEIVED.

Chapters from Modern Psychology. By Professor JAMES ROWLAND ANGELL. Pp. vii + 308. New York: Longmans, Green and Co., 1912. 6*s.* net.

This book is the outcome of eight lectures which were delivered as the first of a memorial series founded to deal primarily with psychological subjects. The author's aim has been "to convey a just and comprehensive impression of the principal features of the psychology of to-day" in language "freed as far as possible from the technicalities of scientific terminology." Accordingly he has provided eight light and entertaining chapters, in an easy-going lecture style, devoted to (i) general, (ii) physiological, (iii) experimental, (iv) abnormal, (v) individual and applied, (vi) social and racial, (vii) animal, and (viii) genetic psychology. A bibliography, almost exclusively American, is given at the end of the book.

Psychology: the study of behaviour. By WILLIAM McDOUGALL, M.B. Pp. 254. London: Williams and Norgate, 1912. 1*s.* net.

This is one of the most useful of the excellent shillingsworths which the Home University Library is providing. Like Professor Angell's book, it contains chapters upon the present position and aims of abnormal, social, animal, individual and genetic psychology. But the entire subject of psychology is here treated far more systematically and thoroughly. The first four chapters, for example, are devoted to the province of psychology, the study of consciousness, the structure of the mind, and the methods and departments of psychology; and all four are admirably written in the writer's very best manner. No higher praise need be given. The book will not only serve as an excellent introduction for beginners, but will help to spread abroad a recognition of the scope of modern psychology, its relative independence of metaphysic and the insufficiency of mechanistic conceptions.

Psychophysik: Darstellung der Methoden experimenteller Psychologie. Von Professor Dr W. WIRTH. (Aus *Handbuch der physiologischen Methodik*, herausgegeben von ROBERT TIGERSTEDT.) S. 522. Leipzig: S. Hirzel. Geb. Mk 20.

This book is an exposition of the most important methods of experimental psychology, in so far as they have not been considered in the portions of this Handbook which have already appeared. After some general questions have been dealt with, nearly two hundred pages are devoted to *Kollektivmasslehre*, using well-known data by Keller, Urban, and others as examples. Here readers will find most of the important mathematics of the statistical methods of psychophysics, with worked examples and clear diagrams, and all expressed according to one nomenclature and one system of symbols.

Portions of this section are original—e.g. the application of Fourier series (p. 66)—and others are fresh in treatment. In the remarks on interpolation by graphical methods on page 51, § 4, it might have been added that there are many instruments on the market which enable integral and differential curves to be drawn direct from given curves. No reference is apparently given to the calculation of correlation factors and the like.

Parts III and IV are devoted respectively to *Reproduktionsmethoden* and *Reaktionsmethoden*. By reproduction methods the author understands such methods as depend upon the description, by the subject, of his experience, in so far as it is the meaning of such description that is important and not merely the vocal reaction. They include threshold measurements, measurements of the field of attention in sight, hearing, etc., memory experiments, experiments on the perception of time, the feelings and the will. The discussion of threshold measurements takes up many pages. Part IV is chiefly a description of the apparatus and practical methods employed in 'reaction' experiments, including the use of the ergograph, tachistoscope, apparatus for the registration of various movements, pneumograph, plethysmograph, etc.

An index would materially increase the usefulness of the book, and a list of the figures (of which there are 63) and of the pages on which they occur would save time in reading. A table of the numerous abbreviations would be useful for those wishing to consult some special section without reading the whole book.

The Nervous System: An elementary handbook of the anatomy and physiology of the nervous system for the use of students of psychology and neurology. By Dr JAMES DUNLOP LICKLEY. Pp. xii + 130. London: Longmans, Green and Co., 1912. 6*s.* net.

The psychologist will find the book useful mainly because of the numerous anatomical illustrations, borrowed from Cajal, His, Testut, Schäfer and Gray, which are here collected in a convenient form. The book cannot be recommended for the physiological information which it professes to offer.

The Gateways of Knowledge: an introduction to the study of the senses. By J. A. DELL, M.Sc. (Vict.). Pp. xii + 171. Cambridge: University Press, 1912. 2*s.* 6*d.*

In this admirable little book, belonging to the Cambridge Nature Study Series, a very successful attempt is for the first time made to provide teaching in experimental psychology for school children from 12 to 15 years of age. The eighty-nine experiments are described with such clearness as to be intelligible even in the absence of the teacher; they involve only the cheapest and simplest apparatus; and must prove of the greatest interest alike to the teacher and to his pupils. The problems and exercises appended to each chapter are particularly useful and suggestive. It is to be hoped that many teachers will give the book a trial.

The Psychology of the Religious Life. By Professor GEORGE MALCOLM STRATTON. Pp. xii + 376. London: Allen and Co., 1911. 10*s.* 6*d.* net.

An attempt is here made, says the writer, "to describe some of the more significant features of religion, and to discover the causes that give them their peculiar character."

Present Philosophical Tendencies: a critical survey of naturalism, idealism, pragmatism and realism, together with a synopsis of the Philosophy of William James. By Professor RALPH BARTON PERRY. Pp. xv + 383. New York: Longmans, Green and Co., 1912. 10*s.* 6*d.* net.

This is a most interesting book, simply and clearly written by an exponent of one of the numerous philosophies now current, which are grouped together by the common label the "new realism." The sub-title of the book indicates sufficiently the contents of the various chapters. It provides a valuable *critique* of present philosophical tendencies.

The Science of Logic: an inquiry into the principles of accurate thought and scientific method. By Professor P. Coffey. In two volumes, pp. xx + 445, vii + 359. London: Longmans, Green and Co., 1912. 7*s.* 6*d.* net. per volume.

This work, written by the Professor of Logic and Metaphysics in Maynooth College, is primarily intended for the use of students preparing for the examinations of the National University of Ireland. It "attempts, in the first place, to present in a simple way the Principles of the Traditional Logic expounded by Aristotle and his Scholastic interpreters; secondly, to show how the philosophical teachings of Aristotle and the Schoolmen contain the true basis for modern methods of scientific investigation, inductive no less than deductive; and finally, to extend, rather than supplement, the traditional body of logical doctrine by applying the latter to some logical problems raised in more recent times." The author insists that he "is quite unconscious of having said or intended anything new or original."

Vom geistigen Leben und Schaffen. Von Carl Becker. S. 164. Berlin: Hugo Steinitz Verlag, 1912. Mk 1.50; geb. Mk 2.25.

This book consists of a series of essays dealing with such subjects as egoism and altruism, morality, freedom of the will, art, language, genius, and the relation of metaphysical problems to current scientific theories. The author's philosophy is a scientific monism.

Essais de Synthèse Scientifique. Par Eugenio Rignano. Pp. xxxi + 294. Paris: Félix Alean, 1912. 5 fr.

A series of essays on consciousness, "biological memory," religion, socialism, and other subjects of biological and sociological interest.

Free Will and Human Responsibility: a philosophical argument. By Professor Herman Harrell Horne. Pp. xvi + 197. New York: The Macmillan Co., 1912.

A Mathematical Theory of Spirit: being an attempt to employ certain mathematical principles in the elucidation of some metaphysical problems. By H. Stanley Redgrove. Pp. xiii + 125. London: Rider and Son, 1912. 2*s.* 6*d.* net.

Development of the Logos-Doctrine in Greek and Hebrew Thought. By Rev. Dr F. E. Walton. Pp. 100. Bristol: Wright and Sons. 3*s.*, bound, 4*s.*

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

January 27, 1912.	Universal and Particular Meaning, by F. Aveling (introduced by C. Spearman).
	A Simple Form of Memory Apparatus (Demonstration), by J. Kay (introduced by J. C. Flügel).
March 23, 1912.	The Effect of a Scientific Training on the Appreciation of Single Colours, together with a discussion of Bullough's Analysis of Perceptive Types, by E. J. G. Bradford (introduced by W. Brown).
	Binocular and Uniocular Discrimination of Shades of Grey, by Shepherd Dawson.
	Association, by A. Wohlgemuth.
May 18, 1912.	Illusions of Reversible Perspective and Fluctuations of Attention, by J. C. Flügel.
	Spontaneous Drawings of Young Children with Typical Examples, by W. H. Winch.

VOLUME V NOVEMBER, 1912 PART 3

THE RELATION OF INSTINCT AND INTELLIGENCE[1].

BY HENRY RUTGERS MARSHALL.

- *I. Introductory.*
- *II. The objective standpoint.*
  - *The organization of complex animal forms. Instinct actions. The importance of cell instinct actions. Interaction of instinct actions. Adaptation. All animal activity reducible to the instinct action form.*
- *III. The subjective standpoint.*
  - *Instinct feelings. Relation between reflexes and instincts and between consciousness and the field of awareness. Instinct feelings and intelligence experiences involve but one process. Relation of instinct feelings to habit, sensation, inhibition and the self. Spontaneity. Determinism.*
- *IV. Conclusions.*

I.

THE relation of instinct to intelligence presents no serious problem to the 'natural' man. He assumes that his efficient intelligence modifies and guides certain of his bodily acts very much as an individual man modifies the forms and guides the movements of inanimate objects in the world about him. Beyond this, with characteristic self-complacency, he looks upon this efficient intelligence as his own peculiar birthright; overlooking evidence of intelligence in the animals, and interpreting their activities as due to *quasi*-mechanical capacities which he describes as instincts.

Even that 'unnatural' man, the thinker, has been wont to take the same view; which has not been wholly displaced despite the general acceptance of the doctrine of evolution, which so strongly emphasizes the continuity of nature's processes. Until very lately we find it generally assumed that actions guided by intelligence are of a radically different nature from those due to instinct; that 'efficient consciousness' has at some stage of development 'come in,' and that with its advent "not only a new factor but a new method of evolutionary progress is introduced[1]"; a conception which however has lost its significance since the doctrine of the inheritance of acquired traits in its cruder form has been abandoned.

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[1] A paper read before the American Psychological Association, 1912, reopening the discussion of the subject by C. S. Myers, C. Lloyd Morgan, H. W. Carr, G. F. Stout, and W. McDougall, whose contributions are reported in this *Journal*, 1910, III. 209—270. Throughout this paper the page references, unless otherwise specified, refer to this series of papers.

Intelligence, as thus conceived, is a concept derived from the introspective apprehension of what we call the 'efficiency' of the self, being used to describe a capacity that is evidenced by this efficiency. Intelligence is thus a purely introspective, or subjective, concept.

Instinct on the other hand is a purely objective concept[2]. It is one derived from the observation of animal activities from the same standpoint that is adopted by the astronomer in observing the motions of the heavenly bodies. This is indicated by the definition of instinct for which Baldwin, Lloyd Morgan and Stout are responsible, who remark that it is "a definitely biological and not a psychological conception[3]."

In considering the relation of instinct to intelligence we are thus attempting to trace the relation between an objective conception, and an introspective or subjective conception; and in the attempt we find ourselves easily shifting from one point of view to the other without recognising that we do so, and, as a result, becoming involved in the most deplorable confusions of thought.

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[1] Cf. C. Lloyd Morgan, *Habit and Instinct*, 1896, 271; also Prof. Judd, Presidential Address, *Psychol. Rev.*, 1910, xvii. 77 ff. The assumption that consciousness 'comes in' at some stage seems to me unwarranted. On the contrary, I find much reason to hold that some form of psychic existence is always present in connexion with each and every animal activity: and evidently if this is the case consciousness itself cannot serve to *explain* the nature of these activities.

The assumption that a special form of consciousness, similar to what Loeb calls 'associative memory,' 'comes in' at some stage of development seems to me equally unwarranted. Until we have a more thorough knowledge than we now have of the physical basis in the human species of what we men know in reflexion as association and memory, we are certainly in no position to affirm that this process, whatever it may be, is different in kind from that found in the lower forms of animal life. If, moreover, we make such an assumption, we are but led to a deceptive assurance that we have found an explanation where none is given; for we gain nothing by ascribing certain activities to an 'associative memory' for which we have not a shred of evidence apart from the forms of the activities themselves.

[2] Cf. Stout, 243.

[3] *Dictionary of Philosophy and Psychology.*

Instinct should not be contrasted with intelligence; rather should we attempt to correlate the activities which we describe as instinctive, and which I call 'instinct *actions*,' with intelligent *actions*; and correspondingly to correlate our instinct-*feelings* with the *experience* of intelligence.

We are likely to reach better results if we first consider the nature of animal activities with no thought that they have any relation to conscious states, clinging thus carefully to the objective standpoint; and then later, assuming the subjective standpoint, consider the nature of the conscious states related to these activities as they occur in our own experience.

II.

Approaching the subject thus, we discover that while animal forms differ immensely in complexity, nevertheless they all, from those of highest to those of lowest complexity, consist of correlated aggregates[1] of what we call cells. Single living cells may be isolated, and we may therefore look upon the cells of an organism as minute animals complete in themselves, and yet correlated with the other cells of the aggregate in which they appear.

In the complex forms we find these cells 'organized,' i.e. so related that each has its own part to play in the life of the whole aggregate. As the animal forms gain in complexity, we find not a mere numerical increase of aggregated cells, but a differentiation of parts into minor systems which themselves go to form the whole systematized organism, and which vary in complexity and in degree of integration. In the higher animals this formation of minor systems within systems becomes highly complex; the most interesting and most thoroughly co-ordinated of these minor systems being what we call the nervous system, which itself again is found to be a system of minor systems of enormously intricate relationship.

Our present interest in this morphological consideration lies in the fact that the variety of activities observed in animals of different kinds corresponds in a general way with the grade of complexity of their organized systems of systems. In the lowest of animals we find but few forms of activity, determined by a limited number of definite types of stimulation. In the highest forms, on the contrary, we find an enormous variety of activities called out by the most varied of stimulations.

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[1] I use the word 'aggregate' in this connexion because of its convenience although it is not altogether satisfactory; for it suggests that the cells in a complex animal are separate entities like grains of sand, whereas we are not warranted in treating cells of organisms as absolutely separate units in the face of the fact that in many cases they appear to be protoplasmically connected.

In the lowest forms we see clearly that these activities tend to meet the needs of the animal as an individual, or to yield results which are of value to the persistence of the species to which it belongs. We find them direct, non-hesitant, often appearing to be "perfect the very first time," to be noticeably modifiable, and to be evidently determined by structural conditions which are innate and inherited. The most noticeable activities of the highly complex animals, on the other hand, are indirect and unpredictable, and often hesitant; and are markedly modifiable, by this process tending to become more direct and less hesitant.

These differences seem very distinctive until we study the complex forms more carefully, when we discover in them not only the unpredictable, hesitant and modifiable forms of activity, but also certain activities directly related to values for the whole organism or to the survival of its species—activities that appear at first sight to be "complicated reactions that are perfect the very first time" (Driesch) and "practically serviceable on the occasion of their first performance" (Lloyd Morgan), e.g. the activities involved when a man 'ducks' his head to escape a sudden blow. These activities of the higher animals are evidently determined in form by structural conditions which are innate and inherited, as are the activities of the lowest animals. They are said to be determined by instinct, the above quotations being definitions of instinct given by the authorities named in brackets. Activities of this type I call 'instinct actions.' The number of diverse instinct actions of this type increases as the animal increases in complexity; and in man we find more of them than in any other animal[1].

It must be noted, however, at once that an instinct action, as above defined, is an ideal form which is closely approached only in the lowest of animals, but of which we find it difficult to discover an example in any of the higher animals; in all of whose instinct actions we find some indications of dependence upon previous activities which is overlooked when we say that their performance is perfect the first time, for few of them are fully serviceable until they have been modified in minor degrees by practice.

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[1] In this I am in hearty agreement with McDougall, 259 ff.

But when we compare the higher with the lower organisms, we find in the higher certain minor systems quite comparable in complexity with the main systems of some of the lower animals. They may be thought of as lower organisms which live a more or less independent existence within the main system. And the activities of these minor systems are found to resemble in nature the instinct actions of these lower organisms. To be sure, they relate only indirectly to the needs of the whole system, but they do relate directly to their own 'minor systems' needs. We speak of these activities as reflexes, of which we have an example in the automatic blinking of the eye. Such activities are evidently due to structural forms that are inherited; they display great directness and a minimum of hesitancy; and in them we find little evidence of modifiability. If we are warranted in describing as instinct actions those activities of the lower organisms with which the activities of these minor systems are comparable, we are certainly warranted, if we cling strictly to our objective point of view, in speaking of the above reflexes as instinct actions of minor systems within the larger system.

These instinct actions of minor systems within the larger system of the higher animals evidently approach more closely to the ideal of an instinct action than do any of the instinct actions of the whole organism. The fact that the nearer we approach simplicity of organization, the closer the instinct actions approach to our ideal, leads us to assume that we should find the ideal form of instinct action if we could isolate and control the stimulation of a biological atom, which for our present purposes we may assume to be a single cell. The activities of such a cell might certainly then be spoken of as a 'cell instinct action'; for if we may judge from the situations noted in the simplest observable forms of life, its activities would be very limited and direct, would be immediate and non-hesitant (although they might be slow), would be determined by innate structural forms due to inheritance, would tend to subserve the needs of the cell and to promote the continuance of its kind, and would be thus not only "serviceable on the occasion of their first performance," but would be "perfect the very first time."

Let us now suppose that a small number of cells, let us say four, are correlated to form an organism of the very simplest form, in which each cell may be supposed to retain its instinctive tendencies. The occurrence of any single cell instinct action will result in an activity of the whole organism of four cells which will be the instinct action of the little organism. Here each cell acts instinctively for itself according to its own nature; but if the aggregate is to persist as such, this 'cell instinct action' must be of such nature that the resultant instinct action of the aggregate subserves the needs of the aggregate. If it does not, the aggregate must die as an organism.

It would appear that the same general process must prevail in the formation of the instincts of the most complex animals. Here however the process will be much more complicated; for the instinct actions of the cell parts of the minor systems within the organism must be coordinated to serve the life purpose of these minor systems which thus display 'minor-system instinct actions'; and these minor-system instinct actions in turn must be coordinated to serve the life purpose of the organism as a whole, before instinct actions serving the needs of the organism as such can occur.

Thus, as organisms gain in complexity, the formation of instincts of general organic import must become more and more difficult. Furthermore, such instinct actions of the whole organism can only be expected to deal with general trends of stimulation. If they are to be effective, they must meet needs of broad significance through great diversity of minor superficial reactions. As animal forms increase in complexity, we should therefore expect to find it increasingly difficult to discover activities of the organism as a whole which approach the ideal of an instinct action. We should expect to find general biological ends attained by many diversely correlated instinct actions, and to note increasing indefiniteness, increasing hesitancy, and increasing lack of predictability of the nature of these reactions. As we have seen, all these expectations are fulfilled.

When an instinct action has been gained by the simplest of organic forms above considered, the individual organism will react instinctively to such normal stimulations as it has become fitted to meet. If however a stimulation abnormal to the type reaches a certain cell of the aggregate, this cell will react for its own life purpose and quite in accord with its cell instinct, but in an unusual manner. This will mean a change in the form of action of the organism taken as a whole. Although the stimulated cell acts quite in accord with its cell instinct, the organism will not act quite in accord with its organic instinct. We shall thus have a modification of the organic instinct action, which, if valuable to the organic individual, will appear as an adaptation to a special need. This, then, may be taken as the typical form of modification of organic instinct actions, and of their adaptation to changed conditions. It merely means that the elemental instinct action is modified in degree to meet a stimulation that is not normal, such modification in degree yielding a modification in the form of the normal instinct action of the aggregate.

As biological systems become more complex and as their varied parts become in themselves more intricately interrelated, organized and self-dependent, these parts must be affected in different ways by any special stimulation, and must often be affected simultaneously by diverse stimuli. Thus different 'cell instinct actions' and 'minor-system instinct actions' must occur together, and diverse parts of the organism must tend to act in diverse ways; the result of which will be the appearance of a great variety of instinct actions of the organism as a whole.

In the higher organic forms, as we have noted, many of the minor systems are so thoroughly organized that they bear much the same relation to the organic whole that the individual cell does to the simple aggregate of cells above described. In such cases these minor systems must be expected to act generally under normal conditions as semi-independent entities, the life of the whole organism being dependent upon the occurrence of their normal instinct actions: such, for instance, is the normal action of the heart in man. But the very fact that these minor systems gain this semi-independence involves the possible occurrence of relatively great disturbances of the reactions of the organic whole of which they are parts, in case the activity of these minor systems is abnormal. Thus the quite natural instinct action of the heart under special stimulation may so modify the instinct actions of the organism as a whole that they no longer meet the needs of the organism as such.

All this shows that in the most complex of animal forms, where the 'minor system' instincts are of great variety, we must look for certain forms of what we call 'adaptation.'

For a given form of stimulation reaching such a complex organism will result in a given form of instinct action; and if this form of stimulation is changed this given form of instinct action will cease. But this does not mean that all instinct actions of the organism will cease; rather does it mean that new forms of instinct action will take the place of the one that has ceased. For what we call the change of form of stimulation may be more properly described as the rise of a new form of stimulation; and where the organism is highly complex this will usually be answered by other forms of instinct action than that called forth by the first stimulation considered.

These other forms of instinct action, if ineffective in relation to the needs of the organism, will appear as mere random movements. If, however, they happen to be effective, we shall have what appear to us, in an objective view, as adaptations of activities to meet special conditions.

Evidently the persistence of an organism will be subserved by such correlations of instinct actions as enable it to meet effectively changes in stimulative conditions as above considered. We should be prepared, therefore, to find just such more or less complete adaptability to meet changes of situation as we discover in the complex reactions of animals of the higher forms.

We turn now to the consideration of the relatively permanent modifications of forms of instinct action during the life of a given organism, which we usually speak of as 'learning by experience.'

When a single cell reacts, we may assume that its structure is in some measure changed; it is difficult to see how it could be held that immediately after its reaction its structure remains exactly what it was before the reaction. Having thus become a new cell form, so to speak, its normal instinct actions will be correspondingly altered. If the stimulus were permanent and if the cell's nature enabled it to maintain the form due to its first reaction to this stimulus, its structure and its instinct actions would be permanently changed; we should have an adaptation of the cell to changed conditions, such adaptation, be it noted, being dependent upon changed structural relations due to its first reaction[1].

In cell aggregates, as we have seen, if any cell or any minor system reacts abnormally, the instinct action of the whole aggregate is altered; which means that the aggregate has for the moment been structurally changed in a manner to fit it to react to the unusual form of stimulus. If we assume that this structural change persists, we shall have a changed system which now reacts differently to a given stimulus. Its forms of instinct action will thus be modified as the result of its previous reaction, and if this new form of reaction subserves the needs of the organized aggregate the latter will flourish and survive. Even if we assume that the simple system thus considered tends to regain the structural condition that obtained before its reaction, it is apparent that the readjustment will require some time; and if a second stimulation of the same kind is received before the old structural form has been regained, the system will be found more ready to act in answer to this stimulation, and this will involve a modification of its instinct action forms. Thus we find it possible to explain the fact that the forms of instinct action in individuals are often noticeably modified as the result of given reactions.

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[1] Cf. Stout, 238.

In very simple aggregates where the structural relations are relatively simple, we should expect to find little evidence of such modifications of instinct actions of the individual. But as animal forms increase in complexity, and the interrelation of their parts becomes more intricate, it would seem probable that readjustments of organic relations within the animal, due to the reaction to a special stimulus, would be increasingly likely to be retained, because the falling back to the structural form existing before the first reaction must then take a very appreciable time. We thus have a possible explanation of the fact that individual adaptations are less observable as we descend from the higher to the lower forms of life; and also of the fact that this adaptation is affected by the frequency, and by the rapidity of recurrence, of the new form of stimulation which produces it[1].

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[1] Prof. Herrick, in the discussion following this paper, suggested that we substitute the terms 'innate actions' and 'individually variable actions' in place of the terms 'instinct actions' and 'intelligent actions.' I see no objection to this use of terms, if it is found to be useful; but I doubt whether such would be the case, inasmuch as in my view both types of action are based on innate tendencies, the individually variable actions being those in which we catch a glimpse of the process by which the instinct action of complex organisms are built up. The distinction which he insists upon is that the individually variable actions are not inheritable, while the innate actions are inheritable. My hypothesis, however, is that all of these individually variable actions are dependent on innate and inherited actions of parts found in special groupings.

This is not the place to discuss the problems of inheritance. I may say however that I have not claimed for a moment that if my view (that complex adaptive intelligent responses are statable in terms of instinct action) is accepted our problem is by any means solved. We merely thus push the problem back in another direction; for it becomes clear that we then have urgent necessity to investigate more fully the basis of the integration of cells which yields the complex organic forms, and the basis of heredity, that determines which of the varied trial responses are to become 'sets,' to adopt Prof. Yerkes's correct phrase.

Of course I agree that the crude hypothesis of the inheritance of acquired traits is utterly untenable; nevertheless I feel that we are here on delicate ground. It appears to me that we are too prone to forget that, in the broadest view, the individual organism is related to the species (less definitely but none the less essentially) as the individual cell is related to the special organism in which it appears. Just as the individual organism is an organic form whose development is conditioned by the formation of new cells having certain functional capacities; so the species is a persistent organic form whose development is conditioned by the appearance of new individuals having certain functional capacities. It seems not improbable therefore that we have in the modification of the

It appears then, from this objective point of view, that the whole gamut of animal activities is explicable on the basis of a unity of biological process; that no line of distinction can be drawn between the process evidenced in hesitant, indefinite, unpredictable and modifiable reactions, and the process evidenced in the most ideal of instinct actions, absolutely immediate, definite, unmodifiable, and predictable; that all varieties of animal activity are reducible to the instinct action form: that all of them are dependent upon structural arrangements which are innate and inherited: that there is no evidence of any influences 'coming in' at any stage of progress to determine modification; that these influences, whatever they may be, are always present in the simplest and in the most complex of animal activities alike.

III.

We have thus far clung to the objective standpoint and have concerned ourselves with the activities of animals only, taking no account whatever of conscious changes correlated with these activities, as they are evidenced in our own introspection and by a process of analogy attributed to other animals.

When, turning from this purely objective study, we consider the relation of our own bodily activities to consciousness as we know it, we note at once that certain of our activities are not felt to involve any changes in consciousness whatever; this being notably the case with those reflexes which in man we have seen approach most closely to the instinct action ideal. On the other hand, we note that certain of our activities are felt to be bound up with, and controlled by, intelligence; and these we discover to be the activities that are farthest removed from the ideal instinct action form. They are hesitant, indefinite, unpredictable by an outside observer; they appear to depend little upon inheritance and much upon practice, and to deal especially with adaptation to special conditions.

What we describe as intelligence, however, is appreciated to be only part of consciousness. Intelligence involves the recognition of ends and of means to the attainment of these ends; but there is a vast body of conscious changes in connexion with which there is no clear

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actions of an individual of a species a complex process corresponding to the relatively simple process of modification of cell action under unusual strength of stimulation. It is with this conception in view that I venture to suggest that in individually variable actions we gain an insight into the nature of the process by which the instinct actions of the complex organism are built up.

appreciation of end or means. Now in this mass of consciousness that seems to lie apart from intelligence we discover certain very specific forms which evidently correspond with activities that we would ordinarily class with the instinct actions. When, without thinking, one 'dodges' the snow ball that some street *gamin* throws at one's head, one is apt to say that one acts instinctively. But in so acting one notes a very special conscious situation which may be described as an 'instinct feeling.'

The activities yielding instinct feelings of this kind, although they evidently have a broad instinctive basis, nevertheless are not found to display the characteristics of the ideal instinct action, as do our so-called 'unconscious' reflexes. Notwithstanding the conclusions gained in our objective view that all activities are of the instinct action type, we are at first inclined to doubt whether we are justified in speaking of such activities, as the dodging of the snow ball, as instinct actions at all.

When, however, we consider the matter more carefully, we note that these complex activities which we commonly speak of as instinct actions, while usually accompanied by what I have called instinct feelings, are at times not accompanied by any observable changes in consciousness at all, and in this come to resemble reflexes. As I walk through the busy street of the city on a cold day, I quite automatically make complex adjustments of my body to prevent me from falling on the icy sidewalks, and this for the most part without my being aware of any corresponding changes in consciousness; although at times, if I happen to slip badly, I am likely to become keenly aware of what I should describe as corresponding instinct feelings.

On the other hand, we note that certain reflexes which we usually think of as 'unconscious,' i.e. which usually are not accompanied by any changes in consciousness of which we are aware, at certain times do yield very marked changes in consciousness; and we note further that when appreciated these changes appear to be of the very same nature as the instinct feelings above referred to. The reflexes which we usually speak of as unconscious, and the complex actions, commonly called instinctive, which are usually accompanied by instinct feelings, are thus seen to be more closely allied than at first they seemed to be.

Facts of this nature remind us that there are many reasons, quite apart from the consideration before us, for holding that the consciousness of each of us is broader than the field of awareness; which in turn suggests that when we describe reflexes as unconscious we mean no more than that their psychic correspondents do not enter the field of awareness, and do not preclude the possibility that they are really accompanied by certain psychic changes which, for some reason, we are unable to discern within that very limited part of our consciousness which we describe as the field of reflexion.

But even if we make the assumption that all instinct actions are accompanied by instinct feelings, only part of which appear in the field of awareness, nevertheless at first sight we see little relation between intelligence and the instinct feelings of which we become aware. We experience an innumerable variety of these instinct feelings even as we act instinctively in an innumerable variety of ways, but these instinct feelings would not be spoken of as intelligent. For intelligence is conceived of as a capacity which enables us to deal with ends, and with means looking to the attainment of ends, and this involves a prevision of our modes of activity; while as Myers has remarked (p. 215) we are, generally speaking, entirely unaware that we are about to act instinctively.

But if we look into the matter with a little more care we are led to question whether, as a matter of fact, the instinct feeling experiences can be said to be entirely lacking in intelligent characteristics.

Certain of these instinct feelings are the accompaniments of very highly complex reactions which are of a relatively definite nature, and occur so often in our experience in powerful form that we have come to recognise them and to name them in very much the same way as we have come to name our sensations. No one who has followed the investigation of the nature of the emotions by James and others can doubt that what we know as the typical emotions—joy, fear, anger, etc., are instinct feelings of this nature.

These special instinct feelings lie clearly within the field of awareness, as do all powerful instinct feelings of the type illustrated above, e.g. in avoiding a blow. But here we have something more than the mere awareness above referred to. For in all typical cases the emotional instinct feeling is involved with an appreciation of an objective situation: joy is bound up with the perception of our approaching friend, fear and anger with an appreciation of the existence of hostile objects or conditions. Evidently this perceptive observation of objective conditions antecedent to the instinct action is not without alliance with what we call intelligence; the act of perception can surely not be said to be sharply distinguished in character from the modes of thought which relate to the appreciation of end and means. Beyond this, although the emotional reactions are not foreseen and determined upon, yet the emotions themselves are scarcely thought of as non-intelligent; and in a measure they are found to be subject to an intelligent control similar to that noted in the higher realms of thought. The boy who whistles to keep up his courage inhibits intelligently the emotion of fear which is quite incompatible with the whistling attitude.

This close alliance between the instinct feelings and intelligence becomes more marked if we take a further step, noting the existence in ourselves of what we may speak of as certain general instinctive trends, which may be evidenced in quite diverse instinct actions; leading us for instance to attack, to flee from, or merely to avoid, an enemy under diverse circumstances. Coordinate with such general instinctive trends we experience what, with Shand, we may call our 'sentiments.' Now when we consider the nature of these sentiments we certainly must hesitate to say that they are non-intelligent. Not only are they often subject to intelligent control, but they often as clearly involve more or less prevision of situations, and at times dim appreciation of ends and means, even though the final emotional reactions be immediate and not recognisedly determined upon.

We thus find the distinction between instinct feelings and intelligence experiences losing its definiteness. Intelligence deals with ends and means which culminate in the attentive emphasis of an idea; this being bound up with instinct feelings corresponding to motor reactions which themselves appear to be quite automatic. Such a situation surely very closely corresponds with that just spoken of in considering the sentiments.

When we consider the experience of clearly distinguished intelligence in itself, we again find the closest kind of bond with the instinct feelings. The experience of intelligence, although much clearer than anything else within the field of awareness, seems to arise out of an inextricable mass of instinct feelings and emotional attitudes, standing out in contrast with all else by its mere emphasis.

All this shows how utterly impossible it is to separate the intelligence experience from instinct feeling. The deeper we make our inquiry the less ground do we find for treating the two as different in kind. This, taken in connexion with the reasons we have adduced for holding that there is but one biological process, surely points to the hypothesis that intelligence, experience and instinct feeling involve but one process, and that the psychic unit, if we may so speak, is instinct feeling, just as the biological unit is instinct action.

It is impossible, in so brief a paper as this must be, even to summarise adequately the evidence favourable to such an hypothesis[1]. It must, however, be noted at once that it finds the strongest possible support when we consider the nature of the mental states corresponding to the acquisition of habits of action. In such cases clearly defined intelligence fades off into instinct feeling in a most instructive manner. In learning to play upon a musical instrument, for instance, the initial activities are voluntary and clearly intelligent. But as practice makes perfect, this recognised intelligence experience gradually gives place to changes in consciousness that cannot be distinguished from instinct feelings, which latter finally fade away into the realm of non-awareness.

The view is further strengthened if we approach the subject from still another direction. It certainly is a significant fact that many powerful thinkers in the past have defended what appears to me the erroneous doctrine that all of our higher mental states are sensational products; for the fact indicates that the simplest and the most complex psychic forms have been felt by these thinkers to display a fundamental unity of process. Now our sensations correspond with instinct actions of complex minor systems within the brain minor system, and from our standpoint must therefore be looked upon as 'minor-system instinct feelings,' just as our emotions are instinct feelings corresponding with instinct actions of the whole organism. The fact that the above-mentioned theory has been defended would thus seem to indicate that ideas, with their essential meaning connotations, have been appreciated by the sensationalists to be of the same nature as the distinctive instinct feelings.

Moreover, this view gains further support, when we consider the nature of other phases of our mental life which are closely related to ideation. When the natural development of instinctive tendencies is inhibited, we should expect to note instinct feelings (corresponding with instinct actions of the general system) tending to remove the inhibition; these we have in our 'impulses[2].' Thus if one's instinctive tendency to strike a snake in his path is inhibited, he experiences not the instinct feeling corresponding with the striking action, but the impulse to strike.

If then ideational situations are of an instinct feeling nature, we should expect, when the natural development of ideas is inhibited, to note in the ideational realm states similar to the impulses, and these we do find in our desires; which are notably similar in nature to our impulses.

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[1] Cf. my *Instinct and Reason* for a fuller consideration of this evidence.

[2] The word 'impulse' is of course used here in the psychological and not in the mechanical sense.

When incompatible impulses simultaneously appear, we experience 'hesitancy'; and where incompatible ideational tendencies simultaneously appear we have 'doubt.' Under such conditions we should expect to note instinct feelings involving the effort of the psychic system to break the deadlock, and these should be found to be closely allied in the instinct feeling and the ideational realms. These we have in the conative struggles to overcome the hesitancy in the one case, and to gain belief in the other. Now the deadlock of hesitancy is broken by the emphasis of one of the instinctive tendencies yielding one of the incompatible impulses; this emphasis being often felt to be due to the influence of the Self. If then the relation of instinct feelings to intelligence is what we are assuming it to be, we should expect to note a correspondence between the resolution of doubt in the field of ideation, and the resolution of hesitancy in the field of instinct feeling. It is significant therefore that we do find this expectation fulfilled; for we discover that as the deadlock of hesitancy is broken by the emphasis of one of the incompatible impulses, resulting in its persistence, so the deadlock of doubt is broken by the emphasis of the stability or realness of one of the incompatible ideas; this emphasis again being often felt to be due to the influence of the Self in the act of belief.

But similar conative tendencies and acts of will are involved in all rational procedure, which is the highest exemplification of intelligence, these involving the development of ideas by the inforcement of that in them which the Self selects. We thus appear to reach the description of the highest forms of intelligence in terms of instinct feeling; for under such a view intelligent procedure consists in the emphasis by the efficient Self of special instinct feelings of great complexity.

And even this experienced efficient Self which thus guides in the rational volitional act may in my view be shown to be nothing more nor less than the most fundamental of all highly complex instinct feelings, corresponding with the most fundamental of all instinct actions representative of all the experience of the race from which we are descended.

If this hypothesis is valid it would mean that the distinguishing marks of the intelligent act are largely, if not altogether, determined by the emphasis of the correlated instinct feelings involved; that they are thus emphasised because of failures of adjustment of the instinct actions with which the instinct feelings correspond; and that in this process of adjustment they are so slowed down that they become related to the prevailing types of mental pattern in a manner that renders them revivable and subject to reflexion. The difference between instinctive and intelligent acts would thus appear to be one involved with an appreciation of temporal situations, somewhat as suggested by Royce[1] in his discussion of the 'time span.' It would mean that if by a slowing down of the process, we could grasp the full psychic significance of an instinct feeling, we should find in it all of the essentials of conation, yielding prospective attitudes, and meaning and the prevision of ends; and that if our intelligent acts could be made immediate they would appear objectively as instinct actions and subjectively as instinct feelings.

If this is true, the study of our more clearly analysable intelligent states should enable us to discern the fundamental characteristics of all psychic process. In this study we discover a sense of 'spontaneity,' a feeling that the ends we strive to attain are of our own making[2]. It would appear then that the germ from which this sense of spontaneity developes must exist in the simplest of all psychic states. And this would mean that some corresponding physical characteristic exists in the ideally simple instinct action.

In our higher mental life we find less and less emphasis of this sense of spontaneity, the closer we approach to the attainment of our ends; and we may surmise that the corresponding physical characteristic is less and less emphasised the closer the instinct action meets the requirements of the organism in which it inheres. We should not therefore expect to discover evidence of its existence in an objective study except where the organism is of high complexity and is subjected to highly complex and variable stimuli; although we might hope to find indirect evidence of its efficiency in the simplest forms through resultants discoverable in the higher forms developed from them.

Now as a matter of fact, we do note in the higher animals a great many forms of activity which seem to have in them new elements; and we discover in organic nature differences of species which can best be accounted for by the hypothesis that organisms have a congenital capacity for variation which, as Stout says (p. 245), is as important to biological theory as heredity. We are dealing here not with something psychic that 'comes in' to modify action, nor with something vital that

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[1] *The World and the Individual*, II. 227 f.

[2] Cf. Myers, 217.

'breaks into' a mechanical system. We are dealing rather with a budding process which is potential throughout, appearing in our psychic life as spontaneity and in organic life as variation.

Until comparatively lately man studied nature under the dominance of the theory that God created the universe in six days and "ceased from his labours." In place of this theory modern science has accepted the conception of a universe in which the varied forms are due entirely to the redistribution of energy, and has usually tacitly assumed that at some time in the indefinite past an unknown something stirred up what was an homogeneity,—that there was a primal ebullition of energy—and that all the phenomena of nature have appeared since that vital moment as the mere result of this primal disturbance. But surely it is much more reasonable to assume that this 'stirring up process,' this process of ebullition of energy, always has been and still is potential throughout nature; that its existence is usually masked by the complexity of the systems observable by us, in which occur many inhibitions of processes due to this creative force; that it becomes efficient only under special conditions, and then yields effects which are so small as to be measurable only under unusual conditions; that this process is however evidenced objectively in organic variation, and subjectively in the sense of spontaneity.

I am a determinist in the sense that I see reason to believe "that if all the conditions determining our behaviour were but given, one result and no other could issue therefrom[1]." In fact I would hold that freedom of the Self is inconceivable unless determination is granted[2]. But the determinist must recognise that we can never discover the nature of all the causal determinants of behaviour; he is in no manner justified in overlooking any evidence of the existence of such a determinant. Just because I am a determinist therefore I feel bound to take cognisance of this sense of spontaneity, and to postulate in organic nature, as objectively viewed, some corresponding characteristic to which we may perhaps be allowed to apply Bergson's phrase *l'élan vital*. Thus although I see no ground for the assumption that consciousness 'comes in' at any stage of organic development, but much evidence that it exists in connexion with all vital phenomena, nevertheless I see strong reason to hold that the psychic existence which corresponds with animal activities is indicative of a process of fundamental importance to organic development.

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[1] Cf. Myers, 217.

[2] *Consciousness*, 641 ff.

McDougall well holds (p. 261), this position cannot be maintained unless Stout holds the untenable view that "the innate factors by which instinct is marked off are the motor mechanisms only."

(C) I find no reason to make a definite distinction between instinct actions and reflex actions. In this Lloyd Morgan (p. 225) agrees with me. Certain of Myers's statements (cf. p. 210) indicate that he would disagree, but his parenthetical sentence (p. 212) grants all I can ask in this particular. Stout disagrees, holding (p. 244) that congenital prearrangements of the neuro-muscular mechanism for special modes of behaviour suffice to explain the reflexes, as is not the case with instinct actions. I find no evidence in either objective or subjective observation to favour such a view. The reflexes appear to me to be merely instinct actions of thoroughly coordinated minor systems within the whole system; minor systems which have gained a large measure of independence. I would not, however, describe instinct actions as 'complex reflexes' (p. 210): rather would I hold that reflex actions are a special type of instinct actions.

(D) Turning finally to the distinction between instinct and intelligence, it is to be noted that all parties agree that intelligent activities are ones that clearly exemplify modification. This fact, however, surely does not warrant us in holding that they are to be sharply distinguished from instinct actions; for the distinction, I contend, would disappear could we observe the instinctive processes with sufficient minuteness, inasmuch as each action of any kind, be it of a complex system, or of a unit, involves a modification of a condition and structural relation which existed previous to the action. In this particular my position is much strengthened by the acute and cogent arguments of Stout (p. 238 f.), who, however, makes a distinction on other grounds to which I refer below.

Myers agrees with me that no distinction between intelligence and instinct is warranted, holding that every intelligent action is also instinctively determined, and that every instinctive action is also intelligently determined. This contention is much strengthened by the arguments of Myers (p. 210 f.), Lloyd Morgan (p. 223 ff.) and McDougall (p. 251 ff.), all of which go to show that meaning and prospective attitude and conative tendency, which are so clearly distinguishable in our intelligent consciousness, are necessarily involved in germinal form in connexion with our instinctive acts. It seems clear to me that even the simplest mental items that I am able to isolate in reflexion display a prospective attitude,—a conative tendency which points beyond itself;

[illegible] systems must necessarily [illegible] a reaction which in [illegible] appears in the form to which we apply [illegible]

[illegible] acts are intelligently determined but [illegible] intelligent acts are not instinctively deter- [illegible] however I cannot see that he has established [illegible] according to [illegible] intelligent con- [illegible] activity separable from the [illegible] and the essential difference [illegible] the possession by the former [illegible] But, as [illegible] the capacity of learning [illegible] an essential feature not only of [illegible] activity so called [illegible] and in this his [illegible]

[illegible] with my position, holds [illegible] instinct to be useful (p. 252), [illegible] being manifested if no modification [illegible] which does not seem to [illegible] above referred to.

[illegible] the distinction between intelligence [illegible] degree, contending (p. 229) [illegible] control system' from [illegible] This contention appears [illegible] to find in the highest [illegible] activities of orders of greater [illegible] of no evidence to indicate [illegible] our attention are not [illegible] organism and I see many reasons [illegible] observation keener, the distinction Lloyd Morgan [illegible] disappear entirely.

(Manuscript received 30 March, 1912.)

THE "PSYCHOLOGICAL INTERPRETATION OF LANGUAGE."

By A. M. HOCART.

Current psychological interpretation of the language of savages—Its neglect of the social context—The social factors underlying the presence or absence of linguistic distinctions—The importance of historical factors.

THE doctrine seems fairly well established among comparative psychologists that savage languages are encumbered with useless distinctions which civilised man has learnt to discard and that they are poor in general concepts and rich in minute subdivisions of the species; dearth of ideas and superfluity of words are their failing.

This view may seem quite impregnable to the thinker at home, and, as it once seemed so to me, I cannot well blame those whose faith has never been exposed to that powerful dissolvent, experience of savage life. A few linguistic facts picked from various treatises and isolated from their vast context, which is no less than the whole life of the people, are just what tends to breed a false confidence of truth secured once for all. Even eminent psychologists have fallen into this pitfall from which few pioneers escape; they will not therefore take it amiss if I give them a caution, and advise them to desist from the psychological interpretation of language unless they are conversant with the life and language of the people. Their knowledge must embrace everything that savages do, or think, or say; for how can we judge of a language till we know the uses it is put to and the thoughts it is required to express?

I do not wish to be dogmatic, or claim to set up a theory of language. I shall merely content myself with showing that some facts quoted in illustration of the orthodox theory can be subjected to a totally different interpretation. I shall take my text from the writings of Prof. Stout. In his *Analytic Psychology* (1902, II. 231) we read:

"The occurrence of distinctions of this kind in savage languages is sometimes regarded as a mark of their superior accuracy and subtlety, as compared with civilised tongues. Thus the missionary Williams speaks of the 'precision and perfection' of certain Polynesian dialects, and refers especially to 'nice distinctions in the pronouns.'" Prof. Stout goes on to quote Williams: "A short time since, I was dining at Bath, when the lady of the house desired the servant to bring a plate, and, politely addressing me, said, 'Put your bones upon the plate, sir.' Now, common as this expression is, it is certainly rather ambiguous. In the language of the Polynesians, however, there would be no such ambiguity, for they have two pronouns to express the difference, *tooe* and *taoe*; the former of which would be used if my own bones were meant; and the latter if those of the pheasant of which I had been partaking." Prof. Stout remarks: "It certainly appears odd that a lower grade of intellectual development should be marked by superior nicety and precision of discriminative thought. The truth is that these distinctions, so plentiful in savage languages, are due rather to an incapacity for clearly apprehending identity in difference, than to a superior power of apprehending difference in identity....Similarly the savage mind has not pushed its analysis far enough to be able to reconstruct certain complex concepts out of simpler. Hence it simply takes the complex concept in its unanalysed totality and marks it by a separate sound."

Now, there is no doubt that the above distinction is in this particular case quite superfluous, but, until we know the whole language thoroughly, we cannot be certain that these two possessives do not sometimes express a vital difference, which we ourselves are compelled to cast into another form. The fact that words are superfluous in one case no more proves that they are unnecessary duplicates than the overlapping of government departments in some matters would justify the suppression of any. Overlapping is a defect of languages, nay of all classifications, of which language is but one particular case.

Suppose a Fijian Williams were to visit England and, returning home, should laud to his countrymen the nice distinctions of which that country's language is capable, he might point out that for the Fijian sentence *au tiko e na imbe*, English has two—'I am sitting *on* the mat,' or 'I am sitting *in* the mat,' as the case may be; Fijian is ambiguous, English is accurate, so accurate as to seat you *in* an armchair, but *on* a chair; Fijian has but one vague '*e*' to serve all turns. A Fijian philosopher might rebuke the Fijian Williams, pointing out that this scrupulousness is excessive and that Fijian is superior in omitting to point out what every man can see for himself; for who can doubt the exact meaning of *au tiko e na imbe?* Would anyone hesitate as to whether I am sitting on the top or inside? We do not require *in* and *on* to tell us how we use a chair and an armchair. Rather these distinctions, so plentiful in English, betray an incapacity for clearly apprehending identity in difference. Fijian is the more advanced since it has a word to express position in the abstract, whereas English has a different preposition for each several position, and has not yet evolved a common term for all[1]. Here a Fijian resident in England, who had read the Fijian philosopher and derived much benefit from his work, might interfere as a third party and argue: "It is true that in the examples adduced a single word would have done, but there are other instances in which it is not so; and it is true that language has been compelled to evolve several prepositions 'in,' 'on,' 'at,' 'by,' etc., because white men make great use of tables, drawers, boxes, machinery, geometry, surveying, literary descriptions, which all require the most accurate definition, whereas we Fijians are but little addicted to description, and that of the baldest kind. Therefore they must keep a good stock of prepositions for technical use, which can also serve for the finest subtleties, even as we do in our own tongue. Thus a man who lies *on* his bed is taking a siesta; if *in* his bed, he has turned in to sleep or is invalided; *we* always lie on our beds. A motor is in town if it is *in* the road, and in the country if *on* the road; *we* have no towns. There is all the difference between a nail *on* the table and one that is *in* it; *we* had no tables till the white man came. Having thus evolved, by some process or other, these particular forms, the white men have apportioned among them the whole realm of position and have left no room for a general term, no indefinite, indeterminate sphere; for they, like all other people, speak for practical purposes and not in order to satisfy psychologists that they possess the analytic faculty. No matter, therefore, whether position is obvious from the context or not, you must define it, because none but definite terms are at hand; we know perfectly well how a man is sitting if a mat is his seat, yet to substitute on that ground *in* for *on* would shock the genius of the language by its incongruity, because *in* has a determinateness incompatible with the present case, and white men do not keep an indefinite preposition merely for the pleasure of it." Thus our Fijian third party; we must try and be as fair as he.

I do not know the language of the Cook Islands, but I do know

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[1] We are compelled to say: Dogs are not allowed *in* or *on* this car; the Fijian *e na ngginggi* would cover all cases.

Fijian, which has exactly the same idiom. Only the number of Fijian possessives is four, not two, and it would seem a hard task to defend them all; there is one for parts of the body (*-nggu*, etc.), one for property (*nonggu*, etc.), one for food (*kenggu*, etc.), and one for drink (*menggu*, etc.). Better plead guilty at once. But Fijian refuses to plead guilty and invokes the following cases in its discharge: *na ndakunggu* is 'my back,' but *nonggu ndaku* is 'my sister-in-law,' because I turn my back to her. *E yasanggu* is 'beside me,' but *e nonggu yasa* is 'in the side of the house which it is my duty to build.' *Mena kai* is 'his mussel to eat' (or rather 'to drink,' as the Fijian says); *nona kai* is 'his shell' which he, or rather she, uses to split leaves for mat-making; *kena kai* is 'his marks' or 'points,' the tokens of which were once presumably shells. *Waina* is 'its juice' or 'milk'; *nona wai* is 'his medicine' which he owns as a leech; *kena wai* is 'the medicine for that disease'; *mena wai* is 'water for him to drink,' or 'medicine for him to take[1].' *Kalou ni nona* and *kalou ni kena* are both 'spirits of his,' but one is a spirit of his property, who brings him abundance of fabrics (*iyau*), and the other is a spirit of his food (*kakana*), who makes him successful in fishing, or hunting, or planting. Here we touch upon the influence of social customs upon language, for there can be little doubt that the separate existence of the property and food pronouns has been strengthened by the fundamental contrast between fabrics and food which are the two great categories of Fijian activity and social intercourse.

Moreover, to isolate, as is usual, the word from its context is certainly unfair to the language; it seems ridiculous to say *nonggu kote* for 'my coat,' but *kenggu mandrai* for 'my bread'; but let us take whole sentences and the matter wears a different aspect: *kauta mai nonggu kote* is 'bring my coat,' while *kauta mai na kenggu mandrai* is 'bring me some bread,' or 'bring me my portion of bread.' If English has different constructions Fijian is entitled to different pronouns; they merely use different means to do the same work.

So far from occupying a sinecure the food possessive has gradually absorbed new functions and greatly extended the sphere of its activity: *a nona nggase* is 'his old man,' *i.e.* 'father'; but *a kena nggase* is 'the elder one.' Fijian has for historical reasons, as yet unknown, developed four possessives among which it has apportioned the whole sphere of possession; the frontiers are often arbitrary; indeterminate cases are treated as if determinate; but what of that? Is it not better than to invent a fifth pronoun for the sake of logic? Are the yams I have planted *nonggu* or *kenggu*? I may possibly never eat them, and shall certainly never eat them all, but as I planted them for me and my family to eat we shall make it *kenggu*. Language is no hair-splitter; it would sooner be inaccurate than stock words for rare cases. So long as in ninety-nine cases out of a hundred there can be no doubt about the right possessive, the hundredth case can take care of itself. We do not keep a generalised preposition simply because we may not be certain at times whether a thing is *in* or *on* a box, and whether the plate in question is a soup plate *in* which, or a flat plate *on* which, we put food.

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[1] This example is given in part by Hazlewood in his Fijian Dictionary.

In Central Asia they have a name for each kind of horse according to the nature and distribution of its colour, but no word for a horse in general[1]. This seems a triumphant proof of analytic weakness in savages; but hold! Do we know everything that an Asiatic thinks, says or does in regard to horses? Tell us that and we may judge; for colour may be all important to them: they may have superstitious or physiological ideas about it, or it may be a privilege of rank, or the price may depend upon it; in fine there may be a thousand and one reasons why colour should make a difference. Do we not use black horses for funerals, and white ones for a coronation? We have quite a number of colour terms reserved almost exclusively for horses, such as roan, dappled, bay, chestnut; lately I came across a French word for a horse with a black head. I think I am right in saying that a horsey person never speaks of a stallion or a mare as a horse.

We may go through life without ever noticing that whereas we have words for a bull, a cow, an ox, a calf, a heifer, etc., we have none for the species except cattle which is collective; in fact the French have had to invent *bovidé* for neutral cases in archaeology[2]. Does that prove that we simply take "the complex concept in its unanalysed totality?" The reason merely is that the *bovidé* is the most important domestic animal, and furthermore that difference of sex makes all the difference in practical use: the mare, the horse, and the stallion are all used for riding, but a bull is kept for breeding, an ox for work and meat, a cow for milk, and the calf gives a special kind of meat. It is only when they are mingled together in a crowd that we need the general term 'cattle.' In the rare cases of uncertainty (and I have never yet come across one) there is no harm done if the beast is called a cow and turns out to be an ox. It would be interesting to find out how a slum boy uses the words.

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[1] See Radloff, *Zeitsch. f. Ethnol.* 1871, III. 285.

[2] My attention was drawn to this word by Mr Marett.

Returning to the language of savages after the sobering experiences of our own, we shall find that the same rules hold good of them. Here is a case which warns us how differences, slight in our eyes, may carry with them a whole train of practical differences for the savage. The Solomon Islands possess a most useful nut, the kanary, which engrosses much of the islanders' interests and fills much of their existence. In those parts investigated by Dr Rivers and myself they distinguished two kinds: the *vino* and the *ngari*; in our eyes it was merely a difference of size, and we might never have considered them otherwise than as large and small specimens had not the natives given us the two words. Yet closely related as they are, they have no common term. Had we proceeded no further, we might have ascribed this deficiency to "an incapacity for clearly apprehending identity in difference." But is it reasonable to suppose that an identity so glaring could not peep through the thin veil of differences? Why should we, white men, start from identity to end in a perception of differences and the savage work in the opposite direction? If instead of contenting ourselves with a vocabulary we inquire into the practical uses of the kanary, we shall be compelled to justify the savages. We found that from trifling differences sprang a host of momentous ones—technical, commercial, and religious: the seasons of the two species do not coincide; they are gathered differently, because the branches of the *vino* will bear a man and the *ngari* will not; they are cracked differently because the *ngari* is larger, and in one island this was the object of a taboo; they are preserved differently: the *vino* is pounded and made into a package, then smoked; the *ngari* is smoked in the shell; the *vino* coming early supplies the offerings of first fruits, while the *ngari*, being abundant, is used for the later smoked-nuts festival; in certain diseases the one is tabooed as food and the other not; the two, in fact, are only identical in the kitchen, and therefore they have but one word for the roasted kernels and puddings of either. A common term would normally be as useless to them as *bovidé* to all of us who are not scientific men.

Take the coconut again: every stage of its growth has a name, and a South Sea language may recognise as many as nine. We need neither exclaim over the richness of their language nor decry their analytic powers. They have done but what we should have done if coconuts grew in Europe. The reason is simply that, whereas other fruits are either ripe or unripe, each stage of the coconut's growth makes a practical difference: in one it is not edible, in the next it may have milk but no flesh; the shell of one is edible, later it is too hard; as it approaches maturity it is drink for man and food for pigs; later it is prized as food and despised as drink; the stage of development has to be ascertained by filipping the nut, because it is opened differently according as the shell is hard or soft. So necessary are these words that the white resident will inevitably adopt into his English such as are of practical use to him.

A great many languages that use the classificatory system have a word for elder and one for younger brother, but none for both; but all students of kinship will know that seniority is rich in social consequences; a man must always know who is his senior and who his junior, and is always able to specify which; he has a collective term for both, however, since a collection of brothers contains elder and younger. On the other hand, we have a word for brother and one for sister, but none that leaves the sex indifferent; savages have such a word[1].

Take again the particles quoted by Prof. Stout (*op. cit.*, p. 230). Are distinctions between "to cut in two with a knife, to shoot off, to cut off with an axe, to break off with the foot, to break off with the hand, to bite off," so superfluous that a special word for each argues a low mental development? especially if we do more justice to them by translating them: "to cut in two with a knife, to shoot off, to chop off, to stamp off, to snap off, to bite off." In dealing with such people we must always remember that they do not live in studies or in towns, retaining a purely theoretic interest in many kinds of action; they are all handy men and jacks of all trades,—handy men in the literal sense, for they have no machinery but do everything by hand; modes of action, therefore, which are unimportant to us are most weighty to them. Thus a yam is not slashed at (*taya*) except by accident or malevolence, but it can be cut lengthwise (*kola*) or crosswise (*suvi*), a matter not indifferent in planting; expert planters slit (*sei*) them part of the way down; before cooking it may be cut up or chopped up (*dhembe*) to boil. To appreciate their point of view one must have tried to live their life, or at least watched them at their work and got interested in the same; failing that, let a man take to the sea or go a-farming and he will soon find that each mode of life has its minute vocabulary that is as necessary as it is rich.

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[1] Cf. W. H. R. Rivers, "The Primitive Conception of Death," *Hibbert Journal*, 1912, x. 401.

I shall leave American Indian particles alone, as I know nothing about that language. I will point out only that Fijian also is rich in them though it uses them for different purposes, but no one will question the need of them: *nunu-va* means to dive for a thing; *nunu-vaka* means to dive with it. If English finds it necessary to use two different prepositions, a savage language has the right to use two suffixes. A Fijian taking the same standpoint as Prof. Stout might, in fact, maintain the superiority of his own language since English has, for instance, a different root to express every idea connected with sight, but no general term, whereas his own tongue has a term as wide as could be desired, which is particularised by means of suffixes and reduplication, thus:

rai: to see, to have eyesight, to look, to appear;
rairai: to seem, appear;
rai-dha: to see;
vakaraidha: to look at, look for, seek;
vakaraitaka: to show;
irairai: appearance.

In fact Fijian words are often so purged of all concreteness that we have not their equivalent for abstraction in English. To the above *rai* we can add *rongo*, which designates both sound and hearing; *ndre* is the action of pulling in the widest sense, without definition of active or passive; the addition of particles defines it as pulling a thing, or being pulled, pulling against each other, confirming, difficult, obstructing, etc.

What a European, continually using a savage language, wearies of is not the excess of minute distinctions, but the vagueness of concepts so wide that they cannot convey the distinctions which he takes an interest in and which alone make conversation worth while to him: he cannot speak of trees without including all plants; he cannot take the birds and leave the bats and butterflies; he has but one word to use whether he is discussing ambition, rivalry, or jealousy: in translating *lialia* into English he must consider whether mad, foolish, simple, idiotic or ignorant will suit the context, because English has no word to express intellectual deficiency in general; after trying 'words,' 'sayings,' 'speeches,' to render *vosa* he may have to adopt 'maxims,' and yet be dissatisfied because it is too precise. The language in the end seems so poor to him, so destitute of specific terms, simply because he insists on saying things that *he* takes a detailed interest in, but not the Fijian, and wants to be minute where a Fijian is quite content with handling in gross. Let him turn to planting, to handicrafts, and war, and he will find as precise and minute a vocabulary as he may require.

Admitting that savage languages are addicted to particular terms and neglect the generic, yet it is strange that this should be cast at them as a reproach when it constitutes one of the chief excellencies of style. Is it not the rule always to use the lowest possible genus, and does not insipidity result from the use of a general term when there is a more particular one? Good prose is to a great extent the use of the exact word, and the exact word is the narrowest that will fit.

Sometimes, indeed, the particular term is so unusual in certain contexts that the general one is not allowed even where it would be the right one. We cannot say to a South Sea Islander, "Come and see me after lunch, or tea, or dinner, or supper"; because all South Sea meals are identical and none of these terms will describe them; yet if we try 'after meal' the King's English takes offence; the only loophole of escape is to use the Polynesian *kaikai* or the slang 'grub.' Dr Rivers points out to me that of old a man might have said 'after meat'; doubtless meals were less highly differentiated then, but according to the orthodox theory we moderns must be going backwards.

Learning a language we use a dictionary, but studying it we must set it aside and observe the practical use of words. For it is the business of dictionaries to define *per genus et speciem*; they must therefore make words represent genera, which, in actual practice, are specific; we are therefore apt to imagine that we are richer in general terms than we really are. A dictionary may define 'bowling' as 'a mode of throwing,' but the rules of the game oppose them to one another, and a man who speaks of a bowler throwing the ball either does not know the game or means it is no-ball. A Martian might draw thence interesting conclusions regarding the analytic powers of French and English; but we know it is all a matter of playing or not playing cricket.

A cock crows, a hen cackles, a pigeon cooes, a jackdaw caws, other birds sing or chirp or warble, but they cannot cry as they all do in Fijian. Is Fijian therefore more advanced in ornithology? On the contrary, it is because they take no interest in birds that they have but one word; our variety of terms is the outcome of abundant bird life, farmyards, egg-eating, shot-guns, singing-birds, pets and poetry. Dictionaries do require a common term for the purposes of definition; they are allowed to say that crowing is the cry of the cock, but fancy our reciting: "This is the cock that cried in the morn."

Wool is a species of hair, but no one ever speaks of sheep's hair. The French *crin* can be subsumed under *poil*, but in practice they exclude one another: a *poil de cheval* comes from any part of the body *except* the mane and the tail.

If I may put it paradoxically, the very indefiniteness of a term constitutes its definition; the fact that it is general makes it quite plain that it is not particular: if a bowler throws, he does not bowl; if a pigeon cries it does not coo; say a hair, and, whatever it may be, it is not wool. Logic assigns one conceptual area to one word, and then subdivides this area into narrower concepts and so on, as Europe is divided into states, states into provinces, provinces into districts. But practice gives to one word the vast uninteresting, indefinite part of a concept and to others the various interesting, definite, analysed portions; for instance, it classifies into throwing in general and bowling, jerking, pitching, flinging, hurling in particular; the whole class is called after its widest member throwing. It is as though our geography should distinguish England, Scotland, Wales, and Ireland, and lump together the unanalysed residue as Europe; yet when we wished to speak of the continent and the British Islands as a whole we also called it Europe.

If our dictionaries, excellent for their purpose, are quite inadequate for the psychological interpretation of language, how much more so are vocabularies of savage languages, which, even when they are not poor, cannot in the nature of things give more than approximate equivalents of native terms. Of the injustice thus done to savage languages here is a striking instance: Williams[1] says Fijians have a word for clapping hands lengthwise, and one for clapping crosswise; now, these definitions of *sausau* and *dhambo* respectively are sufficient for identification, but do not by a long way exhaust the whole connotation of these words: *sausau* is a rapid, high-pitched clap used to mark the rhythm in dances; *dhambo* is a slow, hollow clap which expresses respect for a superior; Fijian no more requires a common term for both than we do for bowing and playing the piano.

Much unnecessary admiration has in consequence been spent upon the supposed richness of savage languages and many undeserved strictures. The same Williams counts fourteen Fijian words for cutting. He does not give the fourteen, but here are as many as I can think of, or find in Hazlewood's dictionary:

ta, sele, koti, musu, dhembe, kola, suvi, sivi, se, tava.

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[1] *Fiji and the Fijians*, 161.

Now it is very true that all of these words are sometimes translated cut, but equally true that they are not confined within the limits of that word: they all overlap with it, but all overlap too with other English words, so that by a different arrangement we can prove the superiority of English, since to every one of the words we have quoted correspond two or three of ours, thus:

sele	to cut, to carve, to amputate, to castrate.	*koti*	to cut, to snip, to shear, to clip.
tá (*ta*)	to cut, to chop, to hew, to fell, to hack, to notch, to slash.	*musu*	to cut across, to break off.
		dhembe	to cut up, to hash, to mince.
kola	to cut to slice lengthwise.	*se*	to rip, to split, to cut in twain along the fibre.
suvi	to cut to slice to carve crosswise.		
sivi	to hack, to chip, to adze.	*tava*	to cut, to operate, to slit.

That is twenty-one to ten: where is the richness of Fijian? and which of the two is more zealous in expressing minute distinctions? such as 'hash' and 'mince,' 'snip' and 'clip.' It may be objected that English has a general term as well as particular ones. Has it? Does a sculptor cut wood, a surgeon cut his patient[1], or a shepherd cut wool? If cut is accepted as the genus, yet Fijian is only inferior in having two terms, *ta* and *sele*, instead of one, as we may expect from a vocabulary framed at a time when cutting with a bamboo knife and cutting with a stone axe were as radically different as chopping and sawing: now that steel knives can be used in both ways we are apt to miss the point of this dualism. Thus we require a knowledge not merely of the present conditions of the race, but also of their primitive technique.

We have so far reasoned as if language sprang, Minerva-like, from the human brain, ready to meet any contingency. But as a matter of fact, each stage of a language is conditioned by an antecedent stage. When we have proved that a word is useful, we have explained its survival but not its origin. If we convict it of uselessness, we may safely predict its death; but the fact that it is useless now, proves nothing about the mental state of the people who still use it: it may have been adapted to a primitive technique, now disappeared; even the fact that it always was useless proves nothing at all, because it may have been forced upon the people by the material at their disposal. Each generation is provided with a certain material to work with, and it makes the best of it; the best may not be very good, but that is no more a proof of low mental development than the use of stone implements is, where iron was never heard of. Fijian supplies us with a good example: it has two main prepositions for our 'in': *e*, in the same place as the speaker, and *mai*, in another place; thus a Fijian says *e Viti* for 'in Fiji,' but *mai Samoa*. Now these prepositions are certainly demonstratives in their origin: *e* is still used to mean here, and *mai* for yonder; it would appear then that to express 'I am in Fiji,' the Fijian began by saying: 'I am here Fiji'; that was out of question when speaking of Samoa, for Samoa is not here but there; he had therefore to say: 'He is there Samoa.' How far Fijians are still conscious of the proximity and distance attached to *e* and *mai* in their rôle of preposition we cannot tell; when that consciousness dies out we may expect to see *e* ousting *mai* or *mai* ousting *e*, unless the Fijians have died out first.

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[1] Dr Rivers points out that he does 'cut,' but only in a highly technical sense, namely for stone.

It is strange indeed that historical conditions, the importance of which is so obvious in explaining our own language, should be completely ignored in the psychological interpretation of savage languages. Suppose a savage accused us of having more words than ideas and quoted in his support 'commence' and 'begin,' 'end' and 'finish,' 'food' and 'nutriment,' 'God' and 'Deity,' 'image' and 'idol,' etc.—"My dear Sir," we should protest, "read your history before you talk. Then you would know that England was conquered by the Normans who spoke French, and that our medieval scholars borrowed many words from the Latin and Greek. Some of these immigrant duplicates are kept up by pedantry; others, like 'nutriment,' have received a scientific meaning, or, like 'beef' and 'mutton,' a practical use." But who knows how many invasions, conquests, migrations, have not swept over Africa, Oceania and America? Yet most English students have hitherto gone on the tacit assumption that languages grew up among savages just like a plant out of the soil, and they confidently explain psychologically a plethora of words, which, when it exists at all, may be due to mixture of races. The four numbers of Fijian they would interpret as a mark of intellectual shortcoming; but it is most likely that they are the outcome of invasions; for neither dual nor trial can be explained by Fijian laws of sound changes, and the dual is in the third person but a replica of the plural. Without entering into details we may represent what may have happened thus: one people said

'he' for 'one,'
'they two' for 'any number';

the other said:

'he' for 'one,'
'they twain' for 'two,'
'they three' for 'three or more.'

Coalescing they produced:

singular, 'he';
dual, 'they twain';
trial, 'they three';
plural, 'they two.'

The abundance of numbers thus developed by accident has been made use of: the trial expresses a small group—the gens, the household, the passengers on a canoe, etc.; the plural expresses an unlimited number —*keitou* is 'we,' the members of the gens, *keimami* is 'we,' Fijians.

The whole historical argument seems so obvious that we are almost ashamed to insist upon it, and hasten to close it with this question: Is it possible from the tangled skein of savage history to unravel the mental processes that have moulded the language and apportion exactly the influence of mind and the influence of material? Certainly not at present; historical research may at any turn belie the psychological inference. A further question is: Would it be the duty of linguistics and ethnology to do so? If the example of geology can be safely invoked we shall answer no: geology does not seek for new physical laws; it applies those derived from the observation of nature to the data of the globe's crust in order to reconstruct the history of the earth. Such also seems to be the function of ethnology, of which linguistics is but a branch: from the facts of language, customs, and religion, to restore the past history of man according to known laws, known because we can see them happening around us, or trace them in documents. Phonetics has begun to achieve this, and there is no reason why the whole of philology and ethnology should not.

(*Manuscript received* 5 *June*, 1912.)

THE RELATION OF MIND AND BODY[1]. I.

BY R. LATTA.

Problem considered as one of method—Parallelist and animist theories—Parallelism an a priori extension of mechanical hypothesis to mind—Animist theories teleological as regards mind—Psychical and physical worlds remain separate on both views—But if they are real systems they must have immanent teleology—Distinction of physical from psychical is a distinction within one system.

THE problem of the relation of mind and body is so complex and comprehensive that it is impossible to discuss it adequately in a short paper. Attempts have been made to circumscribe the problem by limiting it to the relation of mind and body from the standpoint of psychology. But this raises the question: What *is* the standpoint of Psychology? To this question there is no universally accepted answer. Physiology, as well as Psychology, has a right to be heard, and the physiological point of view is almost as difficult to define as the psychological. Indeed some physiologists find it necessary to introduce into the discussion the governing conceptions of Physics and Chemistry. But at least, it may be said, we may deal with the question from a purely scientific point of view, studiously avoiding metaphysics. I am afraid I find it difficult to see how even this can be done. The scientific point of view must be either the point of view of the physical sciences or the point of view of the natural sciences, and there are scientific men who maintain that these are not the same. It may, of course, be suggested that science investigates the relations of phenomena, leaving to metaphysics the problem of things in themselves. But the distinction between phenomena and things in themselves is a metaphysical distinction, based on a particular kind of metaphysical theory. And, apart from that, when it is applied to the problem of mind and body, it unconsciously begs the question. Can we give any sort of satisfactory account of mind as a phenomenon or a process, or a series or combination of phenomena or processes? If we say that we can, we shall be challenged at once, and if we discuss the question we are plunged in metaphysics. In fact, when we consider the various theories of the relation between mind and body which have been set forth, we find that they are all more or less metaphysical, and that they are really governed by implicit metaphysical assumptions of various kinds. In proof of this I need only refer to Mr M^cDougall's valuable book, *Body and Mind*, which frequently and inevitably deals with metaphysical questions.

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[1] Read before Section I at the Dundee Meeting of the British Association for the Advancement of Science, 1912.

This tendency to metaphysics in the discussion of mind and body is due, I think, to the fact that the problem, at the present stage of our knowledge, is to a great extent a problem of method; and it seems to me that the most useful thing I can do is to consider it in this aspect. When we consider the various theories of the relation of mind and body, we find that they may be practically reduced to two main groups, which, following Mr M^cDougall, I may describe as Parallelist and Animist. I am not sure that this classification is complete, for I think there are some metaphysical theories which would hardly come under either head. But it is sufficiently convenient for our present purpose. The Parallelist theories include the epiphenomenal theory, the double-aspect theories, the various forms of psychical monism and the psycho-physical parallelism of Wundt and Münsterberg. The Animist class may be taken as including any form of the interaction theory. Mr M^cDougall maintains that "there is no way of escape from the dilemma—Animism or Parallelism—and that we must accept Animism, if we find the difficulties involved in Parallelism to be fatal to it."

It may be readily seen that the difference between the two kinds of theory is a difference of method. It is significant that the theory of Parallelism was first set forth by the Cartesian philosophers—the Occasionalists, Spinoza and Leibnitz—though in Leibnitz it is a harmony between the perceptions of monads rather than a parallelism between body and mind. Now it is a fundamental doctrine of the philosophy of Descartes that there is a complete opposition between matter, conceived as extension and motion, and mind, conceived as understanding and volition. What the one is, the other is not; and their only unity consists in the dependence of each on God. Spinoza regards them not as externally dependent on God, but as attributes of God, who is the one substance or reality. Each expresses the essence of the one substance, but each expresses it in a different way. Thought is the idea of extension, mind is the idea of body. The whole universe and every part of it has these two aspects. A stone and the scientific idea of a stone, an animal and the scientific idea of an animal, a human body and the scientific idea of that body are in each case one and the same thing in two different aspects. Thought and extension, mind and body, *idea* and *ideatum* are not separate things or substances, but one thing in different aspects. But these aspects or attributes are absolutely separate from one another. There is no interaction between them. They correspond point for point. The whole attribute "extension" is a self-complete and independent system; and the whole attribute "thought" is another self-complete and independent system. No material thing or mode of extension or motion causes or in any way affects an idea; no idea causes or in any way affects a body or mode of extension. Bodies or modes of extension affect only other bodies, ideas or modes of thought affect only other ideas. Thus the whole universe, so far as our human knowledge can grasp it, consists of two entirely independent systems, the elements of which are in each case connected together with an iron necessity, comparable to the necessity with which the properties of a triangle follow from the conception or definition of a triangle; and yet these two systems have an unexplained unity, in that they are aspects or attributes of one and the same substance.

Now the special point of interest for our present purpose is the independence and self-completeness of the two systems. For, leaving aside the peculiarities of Spinoza's metaphysical doctrines, we have here an expression of the mechanical principle of explanation, which is the basis of modern physical science. This mechanical principle is a method or hypothesis by which physical science endeavours to explain material or spatial phenomena, i.e. phenomena occupying space and therefore outside of one another in space. Physical science proceeds on the hypothesis or assumption that all such phenomena may be regarded as practically forming a self-complete and self-explaining world or system. This hypothetical self-completeness of the material system (the system of matter and energy) is negatively expressed by the law of inertia, which amounts to this, that no material thing or particle can be moved or changed from the state in which it is, except by something else which is also material. It excludes, in short, any action of mental, spiritual or other non-material forces upon material things. The same general idea is expressed in a positive form in the fundamental laws of the conservation of energy and the conservation of matter. The sum of the energy and matter of the universe must remain constant, unless new energy or matter were produced by some non-material cause or unless energy or matter were changed into something non-material, which would contradict the law of inertia. If these laws did not hold, we should have to go beyond nature to explain the phenomena of nature, and this would be inconsistent with the fundamental scientific hypothesis. I have referred to the law of the conservation of energy in its popular form; but my interpretation of it becomes, if possible, even clearer when it is expressed scientifically:—the sum of the energy of any isolated material system is constant. It is virtually put in this form by Spinoza, though he had not arrived at the modern conception of energy.

This system of nature has, of course, no physical bounds. It includes men, as well as animals, plants and inorganic objects. In short, it excludes nothing except the non-material. But the non-material, the mental, remains. It cannot be denied; because the basis of the whole procedure, the root of the mechanical method, was the complete cleavage between the material and the non-material. For a time it may have been possible for the physical scientist, intent on the working out of the mechanical hypothesis, to leave out of account the realm of mind. But mind inevitably came to demand some explanation, and the demand became more pressing in proportion to the success of the mechanical method. In dealing with this problem, the most obvious course was to follow unwittingly the example of Spinoza, to regard the world of mind as another self-complete and self-explaining system, and to apply to it by analogy the mechanical method which had been so successful in the investigation of the system of nature. Accordingly we find David Hume describing his *Treatise of Human Nature* as "an attempt to introduce the experimental method of reasoning into moral subjects." The result of this attempt was the Associationist theory of mind, that is to say, the mental atomist theory, which endeavours to explain all the complex phenomena of mind by analysing them into ultimate mental atoms, each completely distinct and separate from every other. These mental atoms, sensations or impressions and their copies or images, are supposed to be compounded either by way of mere aggregation, on the analogy of physical compounds, or (in the "mental chemistry" view of James Mill and John Stuart Mill) by way of fusion, on the analogy of chemical compounds. Now all the Parallelist views of the relation of mind and body have as their basis some such theory as this. It is undoubtedly implied, though it is not clearly expressed, in the theory of Spinoza, and the very conception of parallelism as a point-for-point correspondence between the psychical and the physical seems necessarily to involve it. In all the forms of parallelism body or matter and mind are conceived as two independent realms or systems, with or without an unknowable basis of unity, and corresponding point by point to one another. The two systems may be regarded as equally real, or one of the two may be conceived as a relatively unreal shadow or accompaniment of the other. But the mental system is invariably conceived as scientifically explicable solely on the mechanical principles which are used to explain the physical system. There has, in short, been no essential development in the Parallelist position since the time of Spinoza, with this exception that Spinoza declared thought and extension, as attributes of the one substance, to be each indivisible, and thus gave to each of them a primary metaphysical unity, while for the Parallelists the unity of mind and the unity of matter are not primary, but each is a result of aggregation or fusion.

This extension of the mechanical hypothesis to mind, along with the conception of an independent psychical system which is the counterpart of the physical, is an obvious expedient, a short and easy way of settling a difficult problem. But it is not scientific. It is as much an *a priori* procedure as the animism of the savage or the "forms and faculties" of the scholastic. Let us see what it involves. We begin by maintaining that the physical and the psychical are so sharply opposed that we can never have a true science of physical things, unless we exclude the psychical from every kind of relation to the physical world. It may appear to ordinary experience as if the psychical does act on the physical and the physical on the psychical. But we must pay no regard to that. We must look at the physical world as if there were no such interaction. That is our hypothesis, and it works as regards a physical world thus conceived. We are enabled to construct a science of such a world. We then assume that the same hypothesis, the same method, will work as regards the psychical world. But such an assumption would amaze us if we were not carried away by *a priori* prejudice. The mechanical hypothesis required the exclusion of the psychical elements in our actual experience. Why? Because it would not work if they were included. It surely then seems obvious that we must find some other hypothesis or method for dealing with the disturbing and refractory psychical. And yet we calmly proceed to construct a metaphorical analogue of the mechanical hypothesis and delude ourselves with the idea that by its means we are giving a scientific account of mind. We fashion for ourselves a psycho-physical (or physico-psychical) mind, in much the same way as some of the older economists invented the "economic man." And some of us even admit that this psycho-physical monster has nothing to do with real life. It is indeed amazing. The only possible explanation of such a procedure is that those who adopt it have, either consciously or unconsciously, departed from the position that the mechanical theory or method is a scientific hypothesis, and have assumed without proof that it is an ultimate metaphysical principle, capable of explaining everything in the universe.

The mechanical hypothesis, with its methods of measuring and weighing, and its ideal of reducing all material things to compounds or aggregates of ultimate simple elements of one kind or another, has been applied to living organic bodies as well as to inorganic things. The question whether the hypothesis is adequate in biology is, of course, a problem for biologists, including physiologists, and I am not competent to deal with it from the scientific point of view. But the adequacy of the hypothesis in biology cannot be said to be established; for we find that the biologists are sharply divided in opinion. Some maintain that the hypothesis is adequate and only requires further working out to establish its adequacy, while others hold that it is inherently inadequate and must be supplemented by some sort of vitalist hypothesis. From the point of view of method, the problem is whether what we mean by "life," "growth," "organism" can be adequately explained by the conception of a mere aggregation of ultimate simple elements. The essence of the vitalist position is the contention that in organism an element of teleological ordering must be recognised, that some sort of teleological hypothesis is necessary in biology. As Dr J. S. Haldane put it in his address to the Physiological section of the British Association in 1908: "On the one hand we have accumulating knowledge as to the physical and chemical sources, and the ultimate destiny of the material and energy passing through the body; on the other hand an equally rapidly accumulating knowledge of an apparent teleological ordering of this material and energy; and for the teleological ordering we are at a loss for physico-chemical explanations." This teleological hypothesis is in direct opposition to the mechanical hypothesis as it was originally conceived and as it is usually interpreted, for the hypothetical exclusion of any action of the psychical upon the physical or material had as its aim the rejection of final causes or teleological conceptions in the interpretation of nature. Yet so far as I understand the vitalist hypothesis (e.g. in the form given to it by Professor Driesch) it does not deny the validity of the mechanical hypothesis, but proposes to supplement it in the case of organic beings by the hypothesis of an ordering or guiding principle, an "entelechy." This principle is not a substitute for the mechanical principle, but a supplement to it[1].

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[1] Dr Haldane, however, does not hold the vitalist hypothesis in this form.

In close analogy with the vitalist hypothesis, and on similar grounds, the animist hypothesis regarding the relation of mind and body has been set forth. It is related to vitalism in much the same way as the parallelist view is related to mechanism, and from the point of view of method we may therefore consider vitalism and animism together. The animist, like the vitalist, hypothesis means simply the introduction of a teleological factor. Mr M^c^Dougall, for instance, says (*Body and Mind*, p. 364): "It is just because we have found that mental and vital processes cannot be completely described and explained in terms of mechanism that we are compelled to believe in the co-operation of some non-mechanical teleological factor, and to adopt the hypothesis of a soul." He describes a soul (p. 365) as "a being that possesses, or is, the sum of definite capacities for psychical activity and psycho-physical interaction, of which the most fundamental are (1) the capacity of producing, in response to certain physical stimuli,...the whole range of sensation qualities in their whole range of intensities; (2) the capacity of responding to certain sensation-complexes with the production of meanings, as, for example, spatial meanings; (3) the capacity of responding to these sensations and these meanings with feeling and conation or effort, under the spur of which further meanings may be brought to consciousness in accordance with the laws of reproduction of similars and of reasoning; (4) the capacity of reacting upon the brain-processes to modify their course in a way which we cannot clearly define, but which we may provisionally conceive as a process of guidance by which streams of nervous energy may be concentrated in a way that antagonizes the tendency of all physical energy to dissipation and degradation." And Mr M^c^Dougall endeavours to include the vitalist in the animist hypothesis by conceiving that the soul "may be operative in the guidance of bodily growth, either directly or by means of a general control exercised by it over some system of subordinate psychic agents" (p. 373). The soul also, he contends, "is in some sense a unitary being or entity distinct from all others" (p. 366).

The vitalists and the animists seem to me to be right in insisting on the necessity of a teleological hypothesis for the scientific interpretation of the organic and the psychical. But the actual hypotheses which they suggest are, I think, open to the gravest objections. The mechanical theory rejected explanation by final causes on the ground that it involved an endeavour to explain nature by means of ends or purposes which are beyond nature. A scientific account of nature must be an interpretation of nature by itself and not by its relation to something else. The business of science is to ascertain what things are, and not what uses they may be put to, or what values they may have for minds. Final causes were thus conceived, not as immanent in nature, inherent in the things themselves or in the natural system, but as external and additional, more or less contingent and independent of the things. Now if teleology is to be understood in this sense, it seems to me impossible to dispute the justice of the mechanist contention. If science is possible at all, the interpretation of nature must be self-interpretation. The light of the sun may be useful and valuable to my mind in enabling me to have visual perception of external objects; but if my mind is regarded as an entity outside of the realm of nature, in which sunlight is an element, this use and value of the light is in no sense an explanation of what the light is. But it is just this external conception of final cause that seems to me to be involved in the animist hypothesis and also in that of the vitalists. The entelechy of the vitalists seems to be an ordering, co-ordinating, concentrating principle supervening upon a mechanical system. The soul of the animist is an independent entity, endowed with the function of guiding and directing the course of brain-processes and streams of nervous energy, and of producing, "in response to" certain stimuli, sensation-qualities and meanings. In both cases the teleological factor is placed entirely outside of the mechanical system, either in the entelechy or in the soul. The two realms, the mechanical and the teleological, body and soul, remain as completely apart as the mechanical theory supposes them to be. The only bridge between them is the conception of "guiding and directing" and the conception of "response." The conception of "direction" is simply a reproduction of the Cartesian suggestion of the "direction of the animal spirits," and the conception of "response" is essentially a form of Occasionalism, for it would make no real difference in Mr M^cDougall's contentions if we were to substitute for the words "in response to certain stimuli," the words "on occasion of certain stimuli." The animist hypothesis accordingly seems to require the supposition of an endless succession of miracles. As regards the production of sensation-qualities in response to definite stimuli, it hardly differs from parallelism, and it reduces interaction to an unexplained process of guidance.

The mechanical view, then, seems to me to be right in its denial of an external teleology, a teleology which implies something coming in, as it were, from outside to direct the working of the machine. But such a view of teleology rests on the implied supposition that final causes are always conscious purposes and ends, or (more broadly) that the idea of final cause means that a given material is shaped and directed by something else (call it "mind" or "life" or what you will) to certain ends. This supposition is a very superficial account of the facts. It attributes everything to the shaping and directing agent, and offers no explanation of how it is that various materials are suitable for various ends and unsuitable for others. If we take account of this fact, we shall find ourselves bound to admit that teleology is not external but immanent. The essence of final cause is the relation of means to ends within a system. The means are not a mere given material, and the ends are not merely imposed upon the means, as we seem to impose at our will a particular form upon a soft mass of clay. The form is to some extent immanent in the material; it is what the material has in it to be. The uses of things are not arbitrary or independent of the things themselves: they are part of the nature of the things. Wherever we find things in systematic relation to one another, we find use, fitness, mutual implication, in short, final cause. The ends are not outside the system. They are the complement of the means, determined by the nature of the system itself. If, for instance, we thoroughly understand the cause of any effect, we know the means by which the effect as an end may be produced, whether or not we, as human beings, can actually produce this effect.

I can, of course, only briefly sketch this view, without developing it; but I may illustrate it from Mr McDougall's definitions of a material thing and a soul. "A material thing or being," he says, "is a sum, not only, as J. S. Mill said, of 'permanent possibilities of sensation,' but also of enduring possibilities or capacities of definite kinds of action and reaction upon other material things. In a similar way we may describe a soul as a sum of enduring capacities for thoughts, feelings, and efforts of determinate kinds" (*Body and Mind*, pp. 364, 5). Both of these definitions are teleological, for they imply that the things defined have definite systematic relations, that they are not mere aggregates of totally distinct atoms, but that they are elements in real systems or systems containing elements. Atoms and aggregates of atoms have no definite enduring capacities of specific kinds. They are what they are, and they may combine in infinite ways.

If, then, the mechanical hypothesis implies that the physical world is a real system, it cannot escape the admission of an immanent teleology. But if it does not imply that the physical world is a system, it is not a scientific hypothesis. For all knowledge, all reasoning, presupposes system. You cannot infer from one thing to another, unless you assume that there is a systematic relationship between them. You cannot even frame hypotheses without the presupposition of system; for all hypotheses are statements of problematic or possible systematic relationships, based on similar relationships which have been already discovered.

Now if it be admitted that the conception of system involves immanent teleology, and therefore that teleology of this kind is a feature of the physical as well as of the psychical system, there seems no further reason for maintaining the Cartesian doctrine of an absolute, or a miraculously bridged, gulf between the physical and the mental worlds. And if we set aside this hypothetical gulf, we get nearer to the facts of experience and get rid of innumerable distortions of the facts, which have arisen from attempts to square the facts with our *a priori* theories. What we have to investigate is one system, within which, of course, we may trace lesser systems, just as modern physics and chemistry are coming to recognise a system of the chemical elements as well as systems within the supposed ultimate atoms. Valuable as has been the work of physical science, it has left out of account innumerable characteristics of the physical objects it investigates. These objects have, for instance, the characteristic that they are conditions of the existence of sensation-qualities. They have also the characteristic of being elements in human constructions of all sorts, such as works of art, including architecture, music, painting etc. These are as real characteristics of the physical objects as are their capacities of action and reaction on one another. Yet physical science does not profess to investigate them. And in such cases as those to which I have just referred there is no possible separation of the physical from the psychical factors or constituents of the phenomena. They may be distinguished by a hypothetical mental abstraction; but they cannot be analysed into separate elements, which can again be combined to produce the phenomenon as a result. The object is just as essential a part of the perception as is the perceiving mind; the material of a work of art is just as essential a part of it as the artist's idea; if we withdraw either element the other disappears with it. And what is true in the instances I have adduced holds throughout the whole range of our experience. There is no entirely independent system of mind and there is no entirely independent system of matter. We may, by a process of mental abstraction, endeavour to investigate the physical element in the system apart from the psychical. This, indeed, is what we do when we use the mechanical hypothesis. And, on the other hand, we may make a similar abstraction of the psychical element, as was done by the Cartesians and the English "Empirical" philosophers and psychologists. But we have no right to convert these abstractions into realities, even if we use a teleological hypothesis for the interpretation of the psychical. The distinctions which we make between the physical and the psychical are distinctions within a fundamental unity.

Thus such an attempt as is made, for instance, by Mr M^cDougall, to maintain the independence of the psychical seems to me unsatisfactory. His contention that meaning has no physical correlate in the brain seems to me to be a valid criticism of the mechanical parallelism which holds that there is a point-for-point correspondence between the physical and the psychical. It is so because a meaning is a universal, and because the mechanical hypothesis can consistently admit no universal except a mere aggregate or collection. But this is not evidence for the independence of the psychical. The object of the meaning is not merely my "idea" or thought of the original physical object. It is the physical object as universal and therefore real; though of course it is not the physical object as merely particular, i.e. as abstract, cut off from its real relations within the physical system. To say that the object of a meaning is merely an idea is to leave on our hands all the perplexities and contradictions of a theory such as that of Locke or that of Hume. Without physical things there would be no meanings, and the physical is quite as much an element in the meaning as the psychical is.

My contention, however, does not mean that the physical and the psychical are ultimately merged in one another or lost in some vague unity which abolishes their distinction from one another. It leaves the whole metaphysical problem open and merely asks that we should have full regard for the facts and avoid the complications introduced by bad metaphysics. The very conception of system, which, consciously or unconsciously, is the necessary guiding conception of science, is directly opposed to the merging of one thing in another by the abolition of distinctions. For all science is the finding of distinctions within a unity. It is ordinary imperfect knowledge that lumps things together in a vague superficial unity, with corresponding superficial distinctions. With every advance in knowledge the distinctions between its objects become sharper and clearer and by the very same process of knowledge their real fundamental unities are more fully revealed. In every sphere of knowledge we shall find that the deeper and more fundamental the unities we discover, the sharper and more real are the distinctions. My objection to the theories I have criticised is mainly that they adopt the rough distinction of ordinary practical experience between the physical and the psychical as a real division or separation, and then laboriously try to bring the two severed factors together again by talking of them as "aspects" or by presupposing miraculous interactions between them. Along this line, it seems to me, we can make no progress. But by frankly recognising the facts, and clearing our minds of opinions, we may get rid of a great deal of confusing speculation, and in the slow progress of our knowledge both of the physical and the psychical we may approach towards a satisfactory theory of their inter-relation.

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THE RELATION OF MIND AND BODY[1]. II.

By HENRY J. WATT.

1. *The basis of the problem.*
2. *Presuppositions of the problem.*
3. *Philosophical references.*
4. *Correlation between the psychical and the physical.*
5. *The attributes of sensation.*
6. *Some difficulties urged against parallelism.*
7. *The problem of psychical analysis.*
8. *The laws of psychical fusion.*
9. *The visual systemic sign.*
10. *Sufficiency of positive statements.*
11. *Psychophysical correlation.*

1. *The basis of the problem.* The scientific problems concerning the connexion between mind and body rest ultimately upon knowledge common to us all. One of the first tasks set to the growing mind is that of distinguishing between fact and fancy, between the present and the remembered. In psychology this practical task reappears in the theoretical difficulty of distinguishing sensation by definition from its virtual copy, the mental image. Sensation and image seem to differ from one another in respect of no aspect or attribute. They may be of the same quality; a remembered rose may be as red as a seen one. Their colours may be equally intense, equally extensive, similarly located—identical in all respects. Very often we fail to make a distinction between sensation and image, as in dreams, in hallucinations, and in many of the common perceptual processes of seeing and hearing. But at other times we distinguish them easily, whether by their different behaviour or by their different relations to the will and to attention. Sensations seem to behave in their own way; we must follow their leading: images come and go as we will; they appear

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[1] Read before Section I at the Dundee Meeting of the British Association for the Advancement of Science, 1912.

and change at the bidding of our thought and attention. However this may be, it soon becomes clear to the mind just expanding into a knowledge of the world and of itself that sensations imply the stimulation of the body by some present object, whereas imagery is an inner vision which the mind retains and recalls without the aid of the body, even though the object remembered is gone for ever.

But a more minute examination of the facts inevitably shows that not only the mind's images but all memory and even the most rational processes of thought are in some crude way dependent upon the integrity of the body. Certain parts of the brain are found to subserve the processes of seeing and hearing, and injury to them will make the memory of sight or sound impossible. From this knowledge we may rush to the conclusion that every mental operation, however minute and special, is dependent upon the operation and cooperation of some special mechanism in the brain. But doubts began to rise when we find how difficult it is to say precisely upon what kind of cerebral mechanism the processes of thinking and reasoning depend. The problem then calls for the most impartial and careful study, in which hypothesis and inference are restrained as much as possible and all extraneous argument is excluded.

2. *Presuppositions of the problem.* It is evident that even a temporary solution of the problem *presupposes some sort of satisfactory knowledge of each of the two correlated fields*—the physical and the psychical. In the physical realm this knowledge may be said to have been attained. There uniform and more or less consistent schemes of arrangement and interconnexion of elements and other units have been adopted. We have some general schematic understanding of the anatomical and physiological dispositions of the central nervous system. Although every particularisation of that knowledge is extremely difficult and arduous, it may be presumed that further research will hardly yield results so strange and so surprising that they will not harmonize readily enough with what is known. This must be fairly obvious, I think, in spite of the fact, that we have detailed knowledge only of certain of the simplest and most mechanical of nervous processes—the reflexes—and that apart from certain facts of localisation we know next to nothing about the nervous processes which subserve our most usual mental events.

But if the outlook on the physical side is free and unbounded, it is as yet hardly so on the psychical side. Various causes contribute to maintain the obscurity which prevails. Very extreme views have been held regarding our ability to know about the mind. Some have supposed that the mind is an open book to be read—by its owner at least—without effort or study. How could it have its characteristics at all, he would ask, without his being aware of them? Others have supposed that, far from knowing all, they know and can know nothing about the mind. Knowledge of it could not be expressed in systematic, general, scientific form. How could sensations, feelings, thoughts and efforts possibly be treated as natural objects, to be described, analysed, and classified? Both of these views are really untenable. A middle way must be followed. We must suppose that we are aware of all that passes through the mind, but that we attend to, describe and name mental processes only relatively seldom and then usually without much regard for systematic values. That is, of course, perfectly natural; for we use our minds, as we use the things around us, at first perceptually and practically, and only long afterwards conceptually and theoretically. Our scientific difficulty is, in fact, just this—to learn how to bring about any desired mental process experimentally, how to attend to it, to describe it repeatedly and fully, and to set it into systematic relation to all other mental processes.

This is the more difficult to do, as we naturally tend to notice first only those mental states which are relatively complex, e.g. emotions, thoughts, and memories. These states must first be analysed into their simplest parts before they can be brought into systematic relation to one another. To many the very idea of such an analysis is repellent; they maintain that no analysis is possible; it seems to them to dissipate its own object. To these thinkers the results of analysis seem to be discrete, independent particles, incapable of re-uniting to form the thought, emotion, or memory, from which they were derived. But such a view seems to me to be but the natural result of a method of analysis which proceeds without any sufficient attempt to maintain a corresponding theory of synthesis and interconnexion. There are many difficulties peculiar to such a theory, difficulties which are not at all like those with which we are familiar in physical science. And we must guard against applying to the results of psychological analysis the notions we entertain regarding physical elements. Being unsuitable in many respects, these notions only serve to form a prejudice against psychological analysis. We must establish our notions of psychical elements on a purely psychological basis.

Various schemes for the systematization of experience have been propounded; but none has been definitely accepted. The science of psychology is at present in the unenviable position of having no concentrative hypothesis which is accepted as a skeleton for all text-books and as the ultimate motive of all detailed work and theory. The theoretical advance of psychology must, therefore, lead to results which will transform and enormously extend the outlook of present theory and which will lead to some unity of opinion upon the general lines of theory and exposition. The need for such a theory speedily becomes evident when we approach questions of detail regarding psychophysical correlation.

3. *Philosophical references.* The specially *materialistic* treatment of the mind as a sort of subtle by-product of cerebral activity may be dismissed, as it rests upon a totally mistaken and ignorant view of experience. It may safely be maintained that if there is any realm where interdependence of parts and systematization seem to be patent, that realm is experience. We are apt to look upon experience as a succession of unrelated particles, determined only from the side of the body, because a bodily counterpart or governing influence is often found where we might least expect it, and because experiences seem to affect one another for change little enough, so long as we are not actually enjoying them. After periods of forgetfulness they return to us much in the same relations as we left them in. But this feature need not indicate a lack of interaction between them. The interdependence of experiences may be largely static in character. The sphere of mental dynamics seems to be the consciousness of the moment, where new elements appear and where potent forces continue for a while, using their powers to produce vast changes. How far abroad from the present moment the influence of these forces extends is not yet exactly known.

We may also well refuse to make any reference here to philosophical problems concerning *the nature of reality.* We must accept as valid the naive view that there do exist things that are not wholly to be identified with the momentary contents of the individual mind and are thus far independent thereof. We may also accept the common distinction of this class of things as 'material' or 'physical' from the class of experiences or 'psychical' things. For the problem of the reduction of this distinction does not affect the problem of body and mind. The problem of body and mind is, and will always, from a preliminary point of view, remain the problem of the connexion of these two classes of things—physical, psychical.

4. *Correlation between the psychical and the physical.* Whatever

the nature of the connexion between body and mind may ultimately appear to be, it must rest upon some form of correlation between physical and psychical things. Both psychophysical parallelism and interactionism involve such a correlation, so far as they agree that some or all psychical processes are accompanied by or are evoked by physical processes or *vice versa.*

No one, I think, would venture to suggest, on the basis of positive considerations, that every single aspect of all the physical processes of a single class, no matter how restricted it be, is represented by an exclusive psychical correlative. Chemical and physical knowledge has grown so minute and so dense as to form an effectual barrier to any such attempt. Attempts have indeed been made to correlate molecular complexity with psychical relationships. The occurrence of consciousness itself is sometimes said to be dependent upon the occurrence of molecules of enormous complexity[1]. Hering's theories of adaptation also make some vague reference to processes of assimilation and dissimilation. And certain aspects of colour theory, especially the independence of white-grey-black vision, complementary relations, the facts of colour-blindness and of the distribution of colour sensitivity[2], sometimes elicit speculations regarding molecular relations. But these theories could hardly be said to offer any hope of doing justice to all the features of molecular constitution. The spatial arrangements of the relatively enormous units of the nervous system call, of course, for closer consideration; but even of these only the more central dispositions are usually brought into correlation with experience. The problem of correlation, if it is to be in any sense exhaustively treated, must be stated from a psychical basis.

These various considerations make it clear that the first step towards statement and solution of the general psychophysical problem must be the formation of an exhaustive catalogue of psychical states of all kinds, properly classified as elements, compounds, or other kinds of derivatives. For every one of these and for every distinguishable aspect thereof we must ask whether some satisfactory correlative cannot be found among known or possible physiological processes and their predicable qualities. Unless this is done, I do not think that the problem of the connexion between body and mind can even be raised. Every setting of the problem presupposes at least a temporarily sufficient completion of that task. The greater our success, the more extensive will be the psychophysical correlation be; the limits of correlation will be given by repeated failure to find or to conceive for each mental state a physical correlative. We must take heed, however, lest the problem of correlation change under our hands to the effort to identify the physical and the psychical or to resolve them into one another. The temptation thus to transgress the bounds of the problem is very great. After finding marked traces of correlation between psychical units and physical units, we are apt to look for a correlation between psychical laws and relationships and physical ones, forgetting that the problem of correlation only exists because psychical units differ radically from physical units and yet at the same time seem so to differ from one another in their own specific ways that their differences can be correlated with the steps by which certain physical units differ from one another in their specific ways. Neither the units of either side nor their absolute differences can be compared with one another, but only their relative differences in so far as these constitute a series of a regular nature on either side. If only the regularity, and not the differences themselves, can be compared, neither can any other laws nor relations on either side be brought into parallel except in respect of formal characteristics, such as regularity, unity in difference, etc.

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[1] W. McDougall, *Body and Mind*, 279.

[2] Cp. C. L. Franklin, *Mind*, N.S. 1893, II. 473 ff.

No more facile and useful scheme of psychophysics could be imagined than that of the associationist psychology, which in its essential form must always hold an important place in the field of psychological theory, however far that may ultimately extend beyond the bounds set by the primitive forms of associationism. In the latter the only important distinctions were those of quality and of simultaneity or succession among sensations and images. Quality could be correlated with the varying localisation of function, familiar to common knowledge and later confirmed by the facts underlying the law of the specific energy of the sensory nerves. Different qualities were not thought to be separated by distance or by anything else; they were simply fused by aggregation into a percept; hence the separation by distance of the areas of localisation created no discomfort. If it came to view at all, it did so naturally in the time intervals between successive images. These intervals were, of course, to be correlated with the time taken for excitation to travel from one sensory area to another.

Whatever prominence may now be given to the notion of association, we have already gone far beyond the simplicity of the early psychology of this school. We are familiar with an enormous amount of detail, especially in the field of simple sensation, which was then unknown. But attention has been concentrated mainly upon the discrimination of absolute and differential limits and the registration of the corresponding values of stimulus; the problem of the systematization of the simple sensations has been comparatively neglected. It has been thought impossible and useless; impossible, because the varieties of sensations we find seem so disconnected psychologically that only the accidents of biological development could give them some sort of explanation; and useless, because many ways of classifying attributes seem possible and none seems to add anything to our knowledge of sensation in general. But it must be evident that the true understanding of the physiology of the senses can only come when we have succeeded in classifying sensations and their attributes properly, so as to make them throw light upon the general constitution of experience. Even before we reach this point, we should find that the true classification of attributes makes the physiology of the senses more coherent and acceptable. For the psychological facts are just the key to the arrangement and interpretation of the physiological facts.

5. *The attributes of sensation.* The most direct attributes of sensation are commonly known as quality, intensity, extensity, local sign, duration, and position in time. The presence of intensive differences in all kinds of sensations, with the possible exception of vision, is undisputed. Differences of opinion exist regarding the quality of auditory, kinæsthetic, labyrinthine, and muscular sensations and regarding the extensity of many sensations, especially the auditory (where some recognise it in the form of voluminosity), and the olfactory, kinæsthetic, and muscular (where it seems to be absent). Nativistic and genetic views of the nature of local signature oppose each other and call for different physiological theories. The attributes of duration and position in time are at present perhaps completely in question. They seem at once the most obvious and the most obscure of all. The attribute of duration finds most general acceptance, which is the readier as this particular attribute is felt to be very harmless and unimportant. Position in time presents greater difficulties; as an attribute it invokes the same sort of suspicion as does a nativistic local sign.

The importance of these two attributes must be emphasized at this point. For the question of the simultaneity or succession of bodily and mental correlatives is bound up with them, and unless this alternative is resolved, there can be no hope, at least from the physical side, of extending the theory of psychophysical correlation into a theory of psychophysical continuity. Correlative simultaneity is incompatible with the general principles of physical science and especially with the conception of energy, if we ignore for a moment the difficulty of considering consciousness as a form of energy at all. And if succession be the more probable view, the further question arises: is the succession of bodily and mental states of only one direction, or is it also reversible? But it must be abundantly evident that the alternative of psychophysical simultaneity or succession is not really resolvable. We can hardly hope to succeed in comparing the conscious position in time of two such events as an experience and its accompanying neural excitation. And even if we could, it would avail us nothing. For the presence of a regular interval of time between neural excitation and correlative experience would be absolutely undetectable. As a matter of fact an interval of time does elapse between stimulation of a receptor and the correlative sensory experience; but we are quite unconscious of it and fail to detect it, unless we infer it from the fact that the time interval for different receptors differs or from the latent time of muscular reaction.

Thus it is evident that we must include duration and position in time amongst the attributes of sensation, even if only to provide a basis for the general problem of correlation formulated above. The problem of the alternative of simultaneity or succession does not follow upon the problem of correlation, but must be merged in it. Hence it is possible to omit all reference to time in the statement of the latter problem.

6. *Some difficulties urged against parallelism.* Some form of parallelism may then be said to be by presumption the accepted doctrine. But the more restricted view of interactionism is not without its supporters. It is, curiously enough, at once the easiest doctrine to make plausible and the most difficult doctrine to prove. To make it plausible it is only necessary to pause before the difficulties of psychological analysis on the one hand and to underestimate the possibilities of neural complexity on the other. None of us can really avoid doing either of these things at some point or other. The difficulties of psychological analysis make us incline to believe that the brain sometimes yields us full-grown 'higher' mental states that are unanalysable, or weaves into a unity components that could not be supposed to produce that unity entirely by themselves. And if we succeed in our psychological analysis, we may perhaps too readily

concede that the brain could not possibly contain mechanisms of that peculiar kind which our analysis demands. Those who are sceptically or critically inclined will, therefore, find it hard to abandon the parallelistic view, however strong the evidence against it may be.

This evidence has been recently gathered by W. McDougall and has been admirably expounded in his works, especially in his paper "On the Relations of Corresponding Points of the Two Retinæ[1]," and in his book *Body and Mind* (Methuen, London, 1911). McDougall attempts with the help of the typical, and, of all, the best studied, example of binocular vision to show *that for certain aspects of psychical states—generally their unity amidst diversity of content—no physical correlative is known or conceivable.* I propose briefly to state his arguments and general conclusion in favour of interactionism, and to urge certain considerations which seem to me to make the facts still compatible with the demands for correlation and consequently with the broader views of parallelism.

The arguments are as follows[2]:

(1) "Any illuminated surface appears no brighter (or but very slightly brighter) in binocular than in monocular vision." [Independence and equivalence.]

(2) The facts of Fechner's paradox. [Reconciliation of differences.]

(3) An after-image is much more easily revived by stimulation of the eye it was formed in than of the other. [Independence.]

(4) Binocular flicker disappears at the same rate of alternation of phases as does uniocular flicker and is practically independent of simultaneity or alternation of phases in the two eyes. [Independence and equivalence.]

(5) The facts of flicker-rivalry and of the rivalry frequently observed in binocular colour-mixture and of the volitional predominance of either of two rival fields. [Failure or suppression of reconciliation.]

(6) The independence of the two eyes with regard to the after-effects of seen movement. [Independence.]

(7) The fusion of disparate points in binocular vision and the influence of practice thereon. [Reconciliation of differences.]

(8) The acquired readjustment of corresponding points in certain cases of squint. [Ditto.]

(9) "Perhaps the strongest evidence against the 'common centre' is afforded by the facts of functional blindness of one eye, whether occurring as a symptom of hysteria or induced by hypnotic suggestion." "But how is this dissociation or circumscription effected? The subject himself knows nothing of the anatomy of his brain[3]."

(10) "In certain rare cases a lesion of the visual cortex has produced a small area of blindness in one retina only: a fact fatal to the common view[4]." [Independence.]

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[1] *Brain*, XXXIII. 371 ff.

[2] *Op. cit.* 372. The words in square brackets are added by me.

[3] *Op. cit.* 352.

[4] *Op. cit.* 292.

(11) "It seems that the owl, the frog, the chameleon, and other beasts of prey enjoy binocular vision in spite of the fact that in them the decussation of the optic nerves at the chiasma is complete."

(12) "The hypothesis of the 'common centre' is founded upon a radical misconception of the conditions of fusion of effects of sensory stimuli"—viz. "That the sensation evoked by the stimulation of any sensory point or nerve-fibre comes into existence as an isolated or detached fragment of psychical existence, and that such fragments become compounded to form a consciousness" only in virtue of a corresponding fusion of subservient nervous conditions. The true statement may be formulated in the following way: "In so far as sensory stimuli affect consciousness, they produce partial modifications of the complex but unitary whole of consciousness; and when several stimuli simultaneously affect consciousness, their effects in consciousness can only be discriminated from one another in so far as there obtains some special ground of distinction. Such special grounds are of two principal classes—namely differences of quality and differences of local signature of the several sensory effects of sensations, as we commonly call them; and the power of distinguishing...sensation-elements by aid of either of these grounds of distinction depends largely upon previous practice in active discrimination."

"When the effects of two or more sense-stimuli appear in consciousness combined to a common resultant, this is because the separate cerebral processes act upon this one being [call it the soul or what you will] and stimulate it to react according to the laws of its own nature with the production of changes in the stream of consciousness[1]."

I think we may agree that these arguments are decisive against any view that holds that the excitations from the two retinæ impinge upon a unitary, common, centre—a sort of blob of undifferentiated jelly—and are there simply and entirely summated, merged or wrought into one another, as two drops of water that run together. But this view hardly needs such heavy condemnation. For if the unity of the individual consciousness does not involve a punctate cerebral seat for the soul—as the failure to find one seems to show,—neither should the lesser unities of consciousness necessarily imply the existence of corresponding punctate centres. Besides, it is obvious that the existence of such punctate centres of fusion would be the very strongest evidence against parallelism. Even if we suppose that in the common centre the two contributory excitations were merely superimposed without summation, such a centre would be useless, because it would offer no physical correlate to binocular stereoscopic vision. The neural basis of binocular vision is undoubtedly much more complex than has been often rashly supposed.

But having established that the two eyes are in certain respects functionally independent, in others functionally equivalent in spite of independence, in others again irreconcilable, and, finally, in others without doubt extensively and variably reconcilable, both with reference to the accompanying experiences and to the muscular outcome,—must we therefore conclude that the demands of no legitimate form of parallelism can be fulfilled? Are we not rather called upon first to systematize as concisely as possible the very various psychological facts adduced, and then to find or conceive some correlative neural basis for them. If we cannot pull the child's coat on to the man's back, we must cut a new garment from our cloth.

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[1] *Op. cit.* 298.

7. *The problem of psychical analysis.* It is a common objection which McDougall urges, that sensations of similar quality fuse in a way that defies analysis. They can be distinguished when they occur successively merely because of their succession. But we must be careful that we do not try to dictate the facts. One obvious limit is set to analysis. We cannot maintain binocular synthesis and at the same time somehow separate it for observation into its discrete parts. But that does not mean that when fusion of sensations takes place, there is no longer any evidence of the existence of the manifold that fused to unity. The fused unity does not differ radically and in every respect from its components, although suggestions are sometimes given to this effect. The rather narrow limits set to the reconciliation of differences in fusion show this most emphatically.

8. *The laws of psychical fusion.* We must simply recognise the peculiarities of psychical fusion. We must not expect them to be the same as those of physical fusion. The law of the conservation of physical energy involves the summation of fused components; it is in fact a quantitative law. If there is anything of which we may be certain it is that the laws of psychical fusion are not quantitative laws. Can we not recognise a law which might boldly be called the law of the conservation of psychical identity, and which might be formulated in the following words?—When two sensory experiences combine so as to produce a unity in which they are not separately distinguishable, all those attributes of which the same varieties are common to both experiences, are conserved identically, without prejudice to any divergent psychical attachments these identical attributes may possess. I know of no attribute of experience which offers any exception to this law. And I do not at all see that on any view whatsoever demand could be made for the existence of a parallel unification or identification of subservient neural processes. The psychical identification as such is quite compatible with physical

discreteness as such. Physical discreteness of the neural processes which actually subserve equal intensities psychically identified (if we have really established such a thing), would only involve the assumption that the neural correlative of intensity is not the same part of the neural unit, say a series of neurons, which subserves a given sensation, as perhaps the neural correlatives of colour-quality or local sign of that sensation. If the fused psychical complex is subserved by a coordinated neural complex, why should not the simple sensation of many concentrated attributes be subserved in the same way?

But the main interest of the systematic psychological study of binocular vision resides, not in this law of the conservation of psychical identity, but in the laws which govern the fusion of experiences in so far as they differ from one another in some one attribute. In binocular vision uniocular fields which differ in quality, intensity, or local sign are reconcilable under certain circumstances. Differences of quality are sometimes reconcilable, when we get the processes of binocular colour-mixture. Differences of intensity often produce lustrous effects and differences of local sign usually give us stereoscopic vision. All of these binocular effects can also be got uniocularly.

9. *The visual systemic sign.* Moreover, we must not omit to take note of the individual nature of the two fields of vision. It is a familiar fact that if we interchange the right and left eye views of a stereoscopic slide of a simple object such as a pyramid, the previously solid pyramid pointing its apex towards the eyes now seems to be a hollow pyramid whose open base is exposed to view. In both cases accommodation, convergence, and the sum total of excitation are the same. The only difference is the interchange of the two halves of the complex excitation. It is therefore clear that the two eyes or the two fields of vision are not indifferently interchangeable systems. We can, I think, hardly assume that the mere disposition of the cerebral components of excitation produces such a radical reversal of psychical effects without any intermediary being present in the uniocular components of fusion. I propose to call this intermediary—for want of a better name—the 'systemic sign.'

If this sign is conscious it should be introspectible, but it need not be readily so. Its existence is sufficiently guaranteed if it is detectable at all. I think this may be allowed, although the evidence is hardly yet clear. Apart from this, however, and as its existence seems to me to be implied as the ground of binocular reversals, I should like to suggest its use as *a means of explaining the facts of functional blindness* referred to by McDougall in his ninth argument. These facts involve a physiological knowledge of areas of localisation only in the eyes of a theory which ignores or refuses the assumption of systemic signs. If there is a visual systemic sign, it is evident that a patient in hysteria or in hypnosis has a direct means of bringing the field of vision of one eye into relation to suggestive inhibition. The inhibition will naturally apply to all the contents of consciousness that are characterized by the systemic sign; this in turn may naturally be aroused by verbal suggestion or by touching one eye or the like. For it is evident that what is associated with a systemic sign can act as a vehicle to reach the contents of that system rather than those of another. To each visual system we must suppose to be linked in complex ways the muscular and tactual sensations from the same eye, as well as all the conceptual terms applied in intelligent life to these.

May we not further consider the systemic sign as the psychical correlative of the separate central localisations of the effects of stimulation of the two retina? I can see nothing but advantage in doing so.

When the contents of the two fields of vision are identical, the difference of systemic signs seems to be somewhat ineffective. The result of fusion gives chief prominence to the conservation of identity, except in so far as the appearance of flatness is concerned. But it would be rash to assert that when the fields of the two eyes are not in any way distinguishable, binocular vision is then completely identical with uniocular vision. There has been a tendency, I think, to presume upon this view in treating of the relations between uniocular and binocular intensities and the like. If there is no summation of intensities in binocular vision, it does not follow that the unitary binocular field which results is a unity without any included differences. It is only so as regards intensity. The same holds for binocular colour-mixture and for other attributes of visual sensations.

On the other hand, when the two fields of vision are extremely disparate in any respect, the systemic signs become again ineffective; they fail to reconcile these differences and rivalry results, be it rivalry of intensities, colours, or local signs. Reconcilable differences must lie apparently within a certain range, which may vary for each kind of difference, as well as for practice; and certain forms of reconciliation exert a strengthening effect upon the reconciliation of otherwise rival fields, as we find in lustre and binocular colour-mixture, which are both supported by identity of contours and by stereoscopic vision.

10. *Sufficiency of positive statements.* But it would be inopportune to pursue further matters which still await exact determination and formulation. Perhaps enough has been said to indicate methods which seem applicable to the systematization of experience. In face of the statement that a reasonable and sufficient analysis of experience is impossible, more than an indication of methods can hardly be expected. In so far as systematization succeeds, we do not need to go beyond the positive statement of the laws included within it. The law of the conservation of psychical identity does not seem to call for any operative agent. I do not see any reason why we should expect exact statements of the laws of the reconciliation of differences in fusion to involve the assumption of any intermediary. The 'soul' seems an unnecessary postulate, at least within the sphere of sensory experience. Whether it is necessary in other regions of experience it is at present impossible to say with positive assurance. But in so far as these regions of experience present features analogous to those of sensory experience, the postulation of the soul as the agent of the unification of experience hardly seems inevitable. For this purpose the soul seems to be as unnecessary in mental life as are in nature the agent 'fórces' we so often tend to assume as the motive life of her laws.

But although the soul does not seem to me to be in this connexion as necessary and as useful as McDougall would have it, I am sure that in postulating the soul to explain the processes of fusion McDougall has emphasized the problem which on the psychical side first faces every attempt to solve the general psychophysical problem. There can be no doubt that the problem of the understanding of the psychical complex as such is in this connexion chief of all. It would be rash in a short paper like this to presume upon the solution of it. I can only indicate the lines of solution which at present seem promising and worthy. I do not think that we can yet afford to accept any conclusion that the satisfactory analysis of psychical complexes is impossible. Nor do we seem to be helped by postulating a most complex and wonderful agent to relieve us of our difficulties. We must face these difficulties boldly and hope for success in the positive systematization of experience and in the understanding of its complexes in terms of its elements. A quantitative understanding of them is excluded, it is true; but have we therefore in the psychical world no form of insight which convinces and satisfies? Surely we have! If we can spread out the physical world under the microscopic eye of science and gaze upon it through that vision with the full promise, if not already the gift of satisfying conviction, can we not also hope to spread out the psychical world before us and leaven it all with the yet partial insight of our inner vision? We still see only with the eyes of childhood; we have not yet observed and pondered enough. But the practical reason of childhood is already the promise of man's purest understanding.

11. *Psychophysical correlation.* When we turn to the problem of correlation, we must ask at each stage in the progress of psychical analysis whether our results are compatible with what we know about the central nervous system from evidence other than psychological. It is of course permissible, where direct knowledge fails, to speculate on the probabilities of neural arrangements by inference from psychical facts. We might even under certain circumstances speculate upon the probabilities of a wider world of mind from consideration of the larger schemes of the physical world. But it should surely make us call a halt when we find that inferences made from psychical facts to physical probabilities are held to establish a partial lapse of the expected parallelism between the psychical and the physical. Even if we admit the validity of these inferences, must we not agree that they do not in principle really carry us beyond the knowledge of every-day life, that there are two eyes, but only one mind and one muscular response? We cannot suppose that the neural paths from the two eyes are entirely separate over their whole course, but only that they are not coordinated so soon or at the same points as we once believed they were. For coordinated they must be somewhere and somehow, not for the sake of psychical fusion, but merely for the sake of the unity of the muscular response.

This coordination of the muscular response is recognised by many as a fact of unique importance. But its importance must not be exaggerated. It cannot be supposed that confluence in the efferent system is the sole ground of psychical fusion and explains all the peculiarities thereof. For we should then be involved in all the confusions of a 'common centre.' Confluence for the purpose of motor coordination can only be supposed to be a partial condition of fusion, if we consider that the neural substrate of, say, intensity lies where the converging paths are still separate, while the neural substrate of local sign lies near the point of confluence. The neural correlates of the various attributes of one simple sensation would then be spread out in some sort of linear series. No objection would, of course, be offered to such a view from the side of psychological theory. Our only enquiry is whether the demands of correlation can be satisfied or not. It is for the physiologist to say upon the basis of direct evidence whether any proposed neural scheme, devised to explain the facts of muscular coordination or to meet the demands of parallelism, is possible, probable, or necessary.

It is a significant fact that the feature of uniocular experience which shows by far the greatest extent of reconciliation of differences in fusion is its local aspect. There is also clear evidence in the cases of lustre and binocular colour-mixture that the reconciliation of differences of brightness and colour is very much supported both by the identification of local signs and by the reconciliation of their differences. The coordination of muscular response seems therefore to involve the existence of a neural basis which shall bind the efferent system to, or give it some sort of foundation in, the neural correlative of local signs; so that the modification or complication of muscular responses shall help to modify or complicate local signs. In this way we might give credence to all the facts and still deny that the motor coordination is the sole ground of fusion. *It would be a condition of fusion only in so far as it is based upon, and by its changes helps to modify* (other means of modification still being possible) *the local signs*, which, as originally given, or as integratively developed, enter as the attribute of sensations into the components of a complex process of fusion.

Therefore it still seems possible to correlate completely the complex psychical unity of binocular vision, fused according to the particular laws of psychical fusion, with the complex physical unity of binocular stimulation and response, coordinated according to the particular laws of neural coordination.

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THE EFFECT OF ASTIGMATISM ON THE HORIZONTAL-VERTICAL ILLUSION, AND A SUGGESTED THEORY OF THE ILLUSION.

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I. *Method of Experiment.*
II. and III. *First and second tests of the effect of astigmatism.*
IV. *The testing of subjects with and without correcting cylindrical glasses.*
V. *Evidence of a physiological factor other than that of astigmatism.*
VI. *The effect of practice on the illusion.*
VII. *Interpretation of the effects of practice.*
VIII. *The nature of the physiological factor involved in the illusion.*
IX. *Summary of results and conclusions.*

IN a previous paper[1] the writer attempted to show that no psychological theory yet put forward can satisfactorily explain the usual overestimation of a vertical line when compared with an adjoining horizontal. The purpose of the present paper is to adduce reasons for accepting a physiological rather than a psychological cause of the illusion, or at least to show that some physiological factor plays an important part in the illusion.

This idea was suggested to me by the fact that in the case of my first subject W. A. in the previous set of experiments, a notable difference was manifest between the number of times the illusion occurred when the right eye only was used and the number when the left eye only was used. The results for the subject E. R. V. (*loc. cit.*, Tables VI, VII, VIII, and IX) showed a greater illusion for the left eye than for the right in three of the four figures (*B*, *C*, and *D*), the difference being as much as 1·5 mm. in each case, when the vertical was standard[1]. In table XI (*loc. cit.*) subject A. D. showed a greater amount of illusion for the left eye, and subject A. S. for the right eye with all three standards (40 mm., 60 mm., and 100 mm.). I do not wish to put too much reliance upon results gained by the method of mean error, but the agreement of the results of experiments in which three different standards were employed is remarkable.

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[1] This *Journal*, 1912, v. 8–35.

Thus there was enough evidence to suggest that a physiological factor is at work, and with this suggestion in mind, I have since continued the experiments upon other subjects[2].

I. *Method of Experiment.*

The experiments were performed in the same way as those described in the former paper, except that the subjects no longer used both eyes together in any of the sittings, the sole purpose now being to discover any differences between the two eyes taken singly.

One important point had to be borne in mind. It was conceivable that astigmatism might affect the amount of the illusion and so be responsible for the difference between the two eyes. Accordingly the following steps were taken.

1. The effects of artificial astigmatism (produced by cylindrical lenses) were tried.
2. I tested the extent of the illusion with each eye in subjects who were quite free from astigmatism. I was only able to obtain two such subjects, L. B. and C. W. V.
3. I tested subjects having a known amount of astigmatism, with and without their compensating glasses,—subjects C. O. K. and J. S.
4. I tested one subject with a known amount of astigmatism, comparing the difference between the eyes with what it should theoretically be if astigmatism were the cause of the illusion.

It will be convenient to consider first the effect of astigmatism upon the amount of the illusion. It can be shown theoretically that regular astigmatism (*i.e.* where the curvature of the cornea requires correction in a vertical direction) tends to blur a horizontal line and slightly lengthen a vertical one[1]. But if we can show, as I think we can, that the effect of astigmatism cannot account at any rate for all the cases in which the illusion differs for the two eyes we shall have grounds for concluding that some other physiological factor is at work.

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[1] The four figures used were as follows: Fig. A └, Fig. B ┘, Fig. C ┐, Fig. D ┌. When no other figure is specified the reference is to Fig. A.

[2] A difference between the results for the right and left eyes respectively can scarcely be due to other than physiological differences. The difference between the contours of the fields of vision of the two eyes, even if sufficient to have *any* effect, should be a constant difference, *i.e.* should always make the illusion greater for the same eye. But this is not the case. In some subjects the illusion is greater for the right eye, in others for the left.

II. *First test of the effect of astigmatism.*

As so many people are slightly astigmatic, it was highly important to determine to what extent a slight degree of astigmatism can affect the clearness of the kind of lines used. To ascertain this various tests were adopted. In the first test I used a cross consisting of black lines at right angles on a white card. I looked at this cross through cylindrical lenses of various strengths (such as are used to correct astigmatism), thus creating artificial astigmatism. Here I had the kind assistance of Dr A. Bradburne of Southport, who fitted the various glasses upon me without my knowing their strength or nature.

The tests were carried out both with the right and left eyes, taken separately and with both eyes together. The axes of the lenses were placed vertically, *i.e.* the horizontal curvature was the one that was affected. I also wore my usual glasses for correction of myopia. The results were as follows:

Strength of Lens	
+ 1·0 diopter.	No discernible difference either for the right or left eye or both together.
+ 1·5 diopters.	The vertical line slightly paler at first, and possibly broader. But this effect soon disappeared, presumably owing to accommodation.
+ 2·0 diopters.	The vertical line distinctly blurred, and horizontal apparently lengthened.

Dr Bradburne repeated the tests upon himself and in every particular confirmed my own observations. The importance of these tests lies in the conclusion that apparently astigmatism of 1 D or less has no effect upon the clearness of such lines, and that any effect due to 1·5 D *may* be compensated for by accommodation.

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[1] Cf. H. R. Swanzy, *Diseases of the Eye*, 1900, 42, 43.

III. *Second test of the effect of astigmatism.*

Two spectacle frames were fitted, one with a plain lens for the left eye, the other with a + 1·5 D cylindrical lens for the left eye. A series of experiments was then performed similar to those described in my previous paper[1], with instantaneous exposure of the horizontal-vertical figure. Only the left eye was used. At one sitting the subject was fitted with the plain lens in front of the left eye, the right eye being covered: at the next sitting the cylindrical lens was used, and so on. The plain lens spectacles were simply used as a "blind" so that the subject should not know when artificial astigmatism was being caused.

I myself acted as a subject in this set of experiments, as I had been pronounced entirely free from astigmatism. The results, given in Table I, form a striking confirmation of the preliminary tests. The illusion was practically the same whether the + 1·5 D cylindrical lens was used or not, even the totals of numbers of the various classes of judgments being remarkably alike.

TABLE I. *Subject C. W. V.*

Horizontal Standard, 60 mm.

With Plain Lens.				With +1·5 D Cyl. Lens.			
Length of Vertical	Greater	Equal	Less	Length of Vertical	Greater	Equal	Less
60 mm.	26	1	0	60 mm.	27	0	0
59·25 mm.	22	2	3	59·25 mm.	22	4	1
58·5 ,,	14	6	7	58·5 ,,	16	4	7
57·75 ,,	13	3	11	57·75 ,,	8	10	9
57 ,,	7	3	17	57 ,,	4	4	19
56·25 ,,	0	2	25	56·25 ,,	1	2	24
	82	17	63		78	20	60

Vertical Standard, 60 mm.

With Plain Lens.				With +1·5 D Cyl. Lens.			
Length of Horizontal	Greater	Equal	Less	Length of Horizontal	Greater	Equal	Less
60 mm.	1	2	24	60 mm.	0	2	25
60·75 mm.	2	6	19	60·75 mm.	1	1	25
61·5 ,,	6	5	16	61·5 ,,	6	5	16
62·25 ,,	13	5	9	62·25 ,,	11	5	11
63 ,,	12	11	4	63 ,,	12	9	6
63·75 ,,	18	3	6	63·75 ,,	21	3	3
	52	32	78		51	25	86

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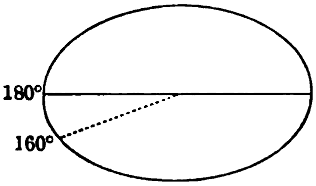
[1] *Op. cit.*

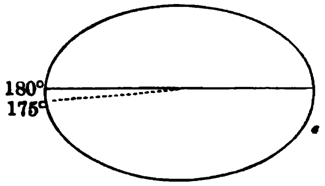
IV. *The testing of subjects with and without correcting cylindrical lenses.*

Under this section appear the results of two subjects, J. S. and C. O. K.

The errors of refraction in the case of J. S. are shown by the following prescription recently given him by an eminent oculist.

Right Eye.		*Left Eye.*	
Spherical	**Cylindrical**	**Spherical**	**Cylindrical**
-0·75 D	**-1·25 D**	**-0·5 D**	**-1·25 D**





The dotted lines show the position of the axis of the lens: the 'abnormal' meridian of the cornea (*i.e.* that which needs correction) is at right angles to the dotted line. Table II shows the results of J. S. (i) with glasses according to the above formula, (ii) without glasses.

The glasses were used at alternate sittings. At the first sitting 'with glasses,' the right eye was tested first and then the left eye, and so also for the first sitting 'without glasses.' At the next two sittings the left eye was tested first, and so on, thus equalising any effects of practice or fatigue.

It will be seen that J. S. shows a 'negative' illusion, *i.e.* he over-estimates the horizontal. The difference caused by the removal of the glasses is small, and what difference there is is ambiguous; the removal of the glasses very slightly decreasing the negative illusion for the right eye, but increasing it for the left eye. Hence in the subject J. S. we have evidence that astigmatism of ± 1·25 D need not affect the illusion.

The results of subject C. O. K. are given in Tables VIII and IX of my previous paper[1]. Her glasses are as follows:

Right Eye.		*Left Eye.*	
Spherical	**Cylindrical**	**Spherical**	**Cylindrical**
-2·25 D	**-1·25 D**	**-2·25 D**	**-1·25 D**

Now this subject does show a difference in the amount of the illusion according as glasses are used or not. The illusion is nearly

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[1] This *Journal, loc. cit.* 14, 15.

2 mm. greater when glasses are not worn. As the axes of the lenses are nearly horizontal[1], we should expect that her astigmatism would, if anything, lengthen the vertical, *i.e.* increase the illusion.

TABLE II. *Subject J. S.*

Standard Horizontal, 50 mm.	(i) With glasses.			(ii) Without glasses.		
	Right Eye.			*Right Eye.*		
Length of Vertical	Greater	Equal	Less	Greater	Equal	Less
52·5 mm.	13	4	7	20	3	4
51·9 ,,	10	3	11	12	8	7
51·3 ,,	8	4	12	8	7	12
50·6 ,,	4	3	17	6	8	13
50·0 ,,	1	2	21	1	9	17
	36	16	68	47	35	53
	Left Eye.			*Left Eye.*		
52·5 mm.	18	3	3	17	5	5
51·9 ,,	9	8	7	6	8	13
51·3 ,,	5	10	9	7	6	14
50·6 ,,	2	8	14	6	5	16
50·0 ,,	2	6	16	4	4	19
	36	35	49	40	28	67

Standard Vertical, 50 mm.						
	Right Eye.			*Right Eye.*		
Length of Horizontal	Greater	Equal	Less	Greater	Equal	Less
47·5 mm.	3	7	14	2	8	17
48·1 ,,	8	6	9	5	10	12
48·7 ,,	12	4	8	9	8	10
49·4 ,,	16	4	4	15	6	6
50·0 ,,	22	2	0	18	6	3
	61	23	35	49	38	48
	Left Eye.			*Left Eye.*		
47·5 mm.	4	7	13	1	3	23
48·1 ,,	5	9	10	6	6	15
48·7 ,,	7	7	10	12	5	10
49·4 ,,	16	3	5	17	6	4
50·0 ,,	20	1	3	24	0	3
	52	27	41	60	20	55

Here then apparently we have an instance in which astigmatism does affect the illusion. Yet the astigmatism is the same in amount as that of subject J. S. and the axes of the lenses are very similar, *i.e.* not far from the horizontal. What is the cause of the difference in the results given by J. S. and C. O. K.? One suggestion occurred to me as the result of an observation when I was a subject for experiments with the cylindrical lenses. I noticed that when using an astigmatic lens, if I did not also wear my usual concave glasses, the horizontal line was distinctly blurred; but this blurring disappeared when I put on my concave glasses. That is, I could correct the artificial astigmatism by accommodation only when my short-sightedness did not also prevent me from getting a clear image of the figure.

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[1] Ten degrees from the horizontal in the case of each lens.

It would seem possible then, when the other optical conditions are satisfactory, that the eye can correct slight degrees of astigmatism which otherwise might cause blurring of one line. Now the one point in which C. O. K. and J. S. differ most markedly is in the degree of their myopia. Whilst C. O. K. has a considerable amount (– 2·25 D), J. S. has very little (– 0·75 D in the right eye and – 0·5 D in the left). I would suggest then that J. S. was able to correct his astigmatism by accommodation and so get a clear image of the figure, whilst C. O. K. could not fully correct it. This surmise is confirmed by the fact that C. O. K. remarked that the figure appeared blurred to her when she did not use her glasses.

V. *Evidence of a physiological factor other than that of astigmatism.*

As I have already remarked, any difference between the amount of the illusion for the right eye and the amount for the left eye must apparently be referred to physiological causes. Now we have seen that astigmatism of ± 1·5 D or less is not likely to affect the amount of the illusion unless it is accompanied by marked defectiveness of vision of another kind. This makes the results of E. R. V. given in the previous paper[1] of significance. In her case the illusion was greater for the left than for the right eye in three of the four quadrant figures, especially when the vertical was standard, the figure then becoming bigger as a whole. (See especially Table IX (*b*) where the judgments "vertical greater" are almost exactly *twice* as numerous for the left eye as for the right. Probably this difference between the two eyes would have been even more marked in this subject if she had been tested by instantaneous instead of by prolonged exposures.) Now this difference cannot, in view of our previous results, be ascribed to astigmatism, as in her case it was very slight, only + 0·5 D for the right eye (axis 45° from the horizontal) and + 0·75 D for the left eye (axis 15° from the horizontal). In other respects her eyesight was excellent[1]. Furthermore, when a cross of two intercepting lines (horizontal and vertical) was looked at by E. R. V. with the left eye, there was scarcely any discernible difference in clearness, the *horizontal* if anything appeared somewhat clearer, whereas with the right eye both were equally clear.

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[1] This *Journal*, *loc. cit.* 19–22.

Important evidence is also afforded by the next two subjects. In the case of C. W. V. there is no astigmatism whatever; yet a marked difference appears between the results for the two eyes, the illusion being twice as great for the right eye as for the left, and three times as great when the vertical is the standard. In the case of subject A. E. T. there is slight astigmatism but it is of such a nature that it should theoretically cause the illusion to be *less* for the right eye than for the left, whereas it is actually *greater* for the right eye[2], and nearly three

TABLE III. *Subject A. E. T.*

Standard Horizontal, 60 mm.

	Right Eye.				*Left Eye.*		
Length of Vertical	Greater	Equal	Less	Length of Vertical	Greater	Equal	Less
60 mm.	10	2	0	60 mm.	9	2	1
59·25 mm.	21	1	1	59·25 mm.	14	6	3
58·5 ,,	17	3	3	58·5 ,,	11	2	10
57·75 ,,	9	5	9	57·75 ,,	4	6	13
57·0 ,,	8	4	11	57·0 ,,	6	4	13
56·25 ,,	7	3	13	56·25 ,,	6	2	15
55·5 ,,	6	2	4	55·5 ,,	1	0	12
	78	20	41		51	22	67

Standard Vertical, 60 mm.

	Right Eye.				*Left Eye.*		
Length of Horizontal	Greater	Equal	Less	Length of Horizontal	Greater	Equal	Less
60 mm.	1	2	6	60 mm.	0	2	7
60·75 mm.	3	3	17	60·75 mm.	6	3	14
61·5 ,,	4	2	13	61·5 ,,	13	1	9
62·25 ,,	5	4	12	62·25 ,,	14	5	4
63·0 ,,	8	4	11	63·0 ,,	16	1	6
63·75 ,,	10	1	12	63·75 ,,	15	5	3
64·5 ,,	13	0	4	64·5 ,,	15	1	1
	44	16	75		79	18	44

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[1] I am indebted to Dr A. McGillivray, Lecturer in Ophthalmology at the University College, Dundee, for his kindness in submitting this subject and also subjects L. B., A. K., J. C. and C. W. V. to a thorough examination for astigmatism and other errors of refraction.

[2] This subject's condition was as follows: Right eye, Spherical − 1·0 D, Cylindrical − 0·75 D, Axis 90° from horizontal. Left eye, Spherical − 0·75 D, Cylindrical − 0·25 D, Axis 20° from horizontal. The astigmatism in the left eye is negligible. So is that for

times as great, when the vertical is the standard. The results of these subjects are given in Tables III and IV.

TABLE IV. *Subject C. W. V.*

Standard Horizontal, 60 mm.

	Right Eye.				*Left Eye.*		
Length of Vertical	Greater	Equal	Less	Length of Vertical	Greater	Equal	Less
60 mm.	30	0	0	60 mm.	24	4	2
59·25 mm.	28	2	0	59·25 mm.	19	8	3
58·5 ,,	25	5	0	58·5 ,,	15	10	3
57·75 ,,	22	6	2	57·75 ,,	2	7	21
57·0 ,,	15	13	2	57·0 ,,	1	4	25
56·25 ,,	9	8	13	56·25 ,,	0	2	28
55·5 ,,	5	6	24		61	35	82
	134	40	41				

Standard Vertical, 60 mm.

	Right Eye.				*Left Eye.*		
Length of Horizontal	Greater	Equal	Less	Length of Horizontal	Greater	Equal	Less
60 mm.	0	0	30	60 mm.	0	4	26
60·75 mm.	0	3	27	60·75 mm.	7	8	15
61·5 ,,	0	2	28	61·5 ,,	11	8	11
62·25 ,,	0	7	23	62·25 ,,	16	5	9
63·0 ,,	3	8	19	63·0 ,,	26	2	2
63·75 ,,	6	8	16	63·75 ,,	27	3	0
64·5 ,,	10	9	11		87	30	63
	19	37	154				

In Table V are given the results of subject L. B., the only other subject who was declared quite free from astigmatism. The amount of illusion is practically the same for the two eyes when the horizontal is standard. With the vertical as standard a slight difference appears, which is made more obvious by the considerable difference between the totals.

In the case of subject C. W. V. extra variables had to be added for the right eye in the course of the experiments. These variables were a vertical line of 55·5 mm. and a horizontal line of 64·5 mm. These were added (without the knowledge of the subject) when it was remarked by the subject that he seemed to be giving far more judgments "vertical greater" than "horizontal greater" with the right eye. This subject was the writer and the apparatus was manipulated by his wife who understood the method thoroughly and had a free hand given her as

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the right, judging from our previous tests. But if it did have *any* effect, it would be to blur the vertical and lengthen the horizontal, *i.e.* to *decrease* the illusion. And yet the illusion is *greater* in the right eye than in the left.

to what variables should be used. In spite of these added variables, the totals indicate an overwhelming majority of judgments "vertical greater" in the case of the right eye, while the opposite obtained for the left eye.

TABLE V. *Subject L. B.*

Standard Horizontal, 60 mm.

	Right Eye.				*Left Eye.*		
Length of Vertical	Greater	Equal	Less	Length of Vertical	Greater	Equal	Less
59·25 mm.	19	5	4	59·25 mm.	24	5	1
58·5 ,,	16	3	10	58·5 ,,	16	11	3
57·75 ,,	13	10	5	57·75 ,,	9	10	11
57·0 ,,	8	6	16	57·0 ,,	7	5	18
56·25 ,,	2	5	23	56·25 ,,	2	3	25
55·5 ,,	0	3	26	55·5 ,,	1	1	28
	58	32	84		59	35	86

Standard Vertical, 60 mm.

	Right Eye.				*Left Eye.*		
Length of Horizontal	Greater	Equal	Less	Length of Horizontal	Greater	Equal	Less
60·75 mm.	4	4	24	60·75 mm.	1	6	24
61·5 ,,	3	7	20	61·5 ,,	3	7	20
62·25 ,,	2	8	21	62·25 ,,	9	7	14
63·0 ,,	9	8	13	63·0 ,,	18	7	5
63·75 ,,	17	6	7	63·75 ,,	22	4	3
64·5 ,,	19	6	5	64·5 ,,	27	0	3
	54	39	90		80	31	69

In view of the difference between the two eyes in the cases of E. R. V., A. E. T. and C. W. V., as shown by the method of right and wrong cases, and also of A. D. and A. S., as shown with three different standards by the method of mean error, it seems likely that the illusion is affected by some physiological factor. Exactly what this factor is we are unable to show upon the data before us. Possibly it is connected with some retinal quality, varying with the part of the retina stimulated. But whatever be its nature, this physiological factor may conceivably be the sole or main cause of the illusion. For, in the case of subjects A. E. T. and C. W. V., the difference between the two eyes is as great as the whole of the illusion for the left eye.

In the experiments described in this paper, the horizontal-vertical figure was fixated at the point of juncture of the two lines. Thus the vertical line would be in the upper peripheral field of vision. If the illusion is due to retinal conditions as suggested, it must be because the same line gives an impression of greater length if seen in the upper than if seen in the right periphery. Recent experiments by Dr H. C. Stevens[1] support this view. He made subjects compare a disc seen by the fovea with similar discs seen in various positions in the peripheral field of vision, on the parallels of latitudes 10°, 20° and 25° from the pole of the visual hemisphere. He found great variations according to the part of the periphery used, but there was a general tendency for a disc to appear larger in the periphery than a disc of the same size as seen by macular vision. What is of special interest for us is that in the case of the two subjects on whom exact quantitative experiments were performed "the enlargement is greatest in the vertical meridian." In the case of both observers the disc in the upper vertical field (on the circle 10° from the line of direct vision) appeared greater than that in the corresponding position of the horizontal field. Stevens found similar variations even in the circle five degrees from the pole. The ends of the horizontal and vertical lines, as seen by my observers, would generally come within one or two degrees of the *ten* degree circle. The illusion would be explained if we suppose that the vertical, particularly the part furthest in the periphery, will be subject to such magnification, more than the horizontal.

Further, there are interesting variations in the apparent size of Stevens's discs. Passing along, say, the vertical meridian, the disc may first appear to increase in size, then to decrease, and then to increase again. It may increase in the vertical and not in the horizontal meridian: or again it may increase in the upper vertical field, but not in the lower. Now if the horizontal-vertical illusion is connected with such retinal variations, we should be prepared to find considerable variations in the amount of the illusion according to the particular quadrant of the field of vision in which the lines of the figure appeared. And this was found to be the case[2]. Moreover, we should be prepared to find that sometimes, even when little or no difference is shown between the two eyes when we are using the horizontal standard and the smaller vertical variables (*e.g.* a horizontal of 60 mm., and a vertical of 55—58 mm.), a marked difference may nevertheless appear between the two eyes when we use a somewhat larger figure consisting of the standard vertical with the larger horizontal variables (*e.g.* a horizontal of 62—65 mm., and a vertical of 60 mm.). This again is what we have found in several cases. (See, for example, results of A. E. T., L. B. and C. W. V. in this paper.)

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[1] *Psychol. Rev.* xv. 69.

[2] Cf. results of E. R. V. in my previous paper. Many such cases also occurred in the series of experiments summarised in Tables XII and XIII in that paper; *e.g.* one subject showed an illusion of 5·1 mm. in Fig. B and a *negative* illusion of 8·1 mm. in Fig. C (av. M.V. 1·99); another, illusion of 3·0 mm. in Fig. C and *negative* illusion of 0·4 mm. in Fig. B (av. M.V. 0·76). Further evidence of variation of this kind in the different parts of the retina is given in a research by Nicolai Poschoga, *Psychol. Stud.* vi. 384. He found that the threshold for discrimination of two points varied greatly with the part of the retina used.

Of course one must not attach too much importance to the fact that in Stevens's experiments the discs were especially magnified in the vertical meridian, since the number of his subjects in the exact quantitative experiments was so small. But they were sufficiently numerous, with the support of quantitative experiments on other subjects, to show the great variability in size-perception in different parts of the retina.

It is not difficult to see that such peripheral variations may greatly influence the apparent length of the lines in the horizontal-vertical figure, so long as the eye fixates the point of juncture of the lines, the vertical line thus lying entirely in the upper periphery and the horizontal in the right periphery[1]. But what happens in the case of prolonged exposure, when the eye moves over the figure? Here, too, it may be urged (*a*) that the eye tends generally to hover about the point of juncture, and then the lines are still within the upper vertical and right horizontal fields respectively; or (*b*) that the eye moves to the end of one of the lines. Suppose the eye to hover about the end of the vertical line; then the vertical line will still remain in the vertical periphery though it is now in the lower half, while the horizontal is still in the right periphery though again it is in the lower half. Indeed, wherever the eye rests, a difference will be possible in the apparent size of vertical and horizontal lines due to retinal differences. But we shall expect this difference itself to vary as the eye moves. And hence (partly) may arise that uncertainty which characterizes the judgments with prolonged exposures. As Dawes Hicks and Rivers remark, "With the prolonged exposures...it was often extremely difficult to come to a decision," and oscillation of judgment sometimes occurred "even when the difference between the two lines was actually at the maximum[2]."

Frequently my own subjects in trying to draw the one line equal to the other have given up the task in despair, saying that they felt their result was not right and yet did not know which way to alter it. Subject E. R. V., with whom prolonged exposures were used[3], remarked that the lines "seemed to change in length" while she continued to look at them. All these observations are more comprehensible if we suppose that the lines vary in their apparent size, according to the part of the retina stimulated by them[1].

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[1] Speaking in terms of the field of vision; of course the upper vertical line will stimulate the lower periphery of the retina.

[2] *Op. cit.* 255.

[3] Subject E. R. V., unlike the subjects of Dawes Hicks and Rivers (*op. cit.* 256) in prolonged exposures, seems to have got rid partially of this oscillation in the course of the

A further experimental confirmation of this theory seemed possible. Subject E. R. V. showed a much greater illusion for the figures A └ and B ┘ than she did for the figures C ┐ and D ┌. This would be explicable if a vertical line in the upper half of the field of vision appeared to her greater than one in the lower half.

Now this proved to be the case for E. R. V. in a marked degree. Using the same apparatus as before, I arranged that a vertical line should appear for a fraction of a second upon the glass screen seen by the subject E. R. V. Near the centre of the line a slight notch afforded a fixation point. The right eye only was used. The exposures were instantaneous, and the subject had to say whether the upper or the lower half of the line appeared the greater. By means of slides, these parts could be varied in length. Table VI shows the results. The lower half was first taken as standard and the upper half varied, and then *vice versa.*

TABLE VI. *Subject E. R. V.*

(*a*) Upper 'half' standard, 45 mm.

Length of lower 'half'	Greater	Equal	Less
50 mm.	4	1	16
51 ,,	5	2	14
52 ,,	8	4	9
53 ,,	11	3	7
54 ,,	16	3	2
55 ,,	17	4	0
	61	17	48

(*b*) Lower 'half' standard, 55 mm.

Length of upper 'half'	Greater	Equal	Less
50 mm.	19	1	1
49 ,,	15	3	3
48 ,,	15	4	2
47 ,,	7	3	11
46 ,,	5	6	10
45 ,,	0	1	20
	61	18	47

It will be seen that, on the average, an upper 'half' of 45 mm. is judged equal to a lower 'half' of 52 mm., a difference of 7 mm. This

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experiments, as she gave no "equal" judgments in the last section (one-third) of the experiments; but she had an exceptional amount of practice, her judgments numbering nearly four thousand five hundred.

[1] When the writer fixates the juncture of the lines with one eye and then slowly moves the eye upwards along the vertical, the upper end of the vertical appears first to approach the point of juncture and then (when the eye has travelled about half-way up) to recede again. The same happens with the horizontal though not to the same extent. The lines should be at least two inches in length and it is well to mark the end with a cross. But this *may* be an illusion of movement, and not due to the passing of the line image over different parts of the retina. It does not, however, seem to occur to the same extent with the left eye as with the right.

is more than ample to account for the difference between the amount of illusion for the figure of the upper 'quadrants' and that for the lower 'quadrants' in the case of E. R. V.

Summing up, we may say that lines of evidence from various points lead to the theory that the illusion is caused by variations in the apparent size of a line according to the part of the retina stimulated.

VI. *Effect of practice.*

The effect of practice in this illusion proves to be highly interesting, but somewhat difficult to explain. One fact is clear—the illusion may actually *increase* with practice, not only with momentary but also with prolonged exposures. No such observation has been made, I believe, in the case of any other illusion[1].

Unfortunately it is impossible to give the practice effects in the case of all subjects, for two reasons. First, it was often impossible to obtain regular sittings, an interval of four days or even a week sometimes intervening between two sittings, so that practice effects were lost. Secondly, it was necessary in several cases in the course of the experiments to add an extra-long horizontal variable (or an extra-short vertical), and to abandon the use of the shortest horizontal (or longest vertical). This had to be done because the illusion proved to be greater, at least after a little practice, than the preliminary tests had indicated, and the excess of judgments "vertical greater" proved harassing to some subjects. This necessary alteration of the lines, though itself possibly an indication of an increase of the illusion with practice, renders impossible a strict comparison of the first set of judgments with the last.

The subject whose sittings were most regular and who had most practice was E. R. V. with whom prolonged exposures were used. With very few exceptions she had two or even three sittings each day. The results are given in Table VII and indicate a marked increase of the illusion with practice, more especially with the right eye. The first set of ten judgments with each variable is taken, and the proportion of "vertical greater" to "horizontal greater" judgments is shown by the figures given.

In Tables VIII and IX I also give the results in the case of subjects W. A. and C. W. V. who also show an increase with practice.

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[1] E. O. Lewis found a slight increase in the Müller-Lyer illusion in two subjects, but only for momentary exposures and during a short series of experiments. See this *Journal*, II. 298.

TABLE VII. *Subject E. R. V.*

Figure A.

		Two Eyes			*Right Eye*			*Left Eye*		
		Vertical greater	Equal	Horizontal greater	Vertical greater	Equal	Horizontal greater	Vertical greater	Equal	Horizontal greater
Standard Horizontal	1st set of 10 judgments.........	22	22	18	30	24	5	35	18	8
	3rd set of 10 judgments.........	39	0	23	50	0	9	42	0	17
Standard Vertical	1st set of 10 judgments.........	26	12	22	35	13	10	37	13	9
	3rd set of 10 judgments.........	46	0	15	49	0	11	48	0	13
Figure C.										
Standard Vertical	1st set of 10 judgments.........	22	23	15	28	11	21	41	6	13
	3rd set of 10 judgments.........	33	0	27	46	0	14	47	1	12
Standard Horizontal	1st set of 10 judgments...	23	20	17	14	19	27	36	10	14
	3rd set of 10 judgments.........	38	1	21	43	0	17	43	0	17

In comparing the figures notice should be taken, not only of the increase of the number of judgments "vertical greater," but also of the decrease in the number of judgments "horizontal greater." In the case of the left eye of subject E. R. V. the number of judgments "horizontal greater" generally increase, thus balancing to a considerable extent the increased number of "vertical greater" judgments.

TABLE VIII. *Subject W. A.*

		Two Eyes			*One Eye*		
		Vertical greater	Equal	Horizontal greater	Vertical greater	Equal	Horizontal greater
Horiz. Standard	1st half of experiments	20	17	35	28	14	30
	2nd half of experiments	36	7	30	28	12	33
Vertical Standard	1st half of experiments	36	15	31	22	20	30
	2nd half of experiments	46	14	21	45	12	15

TABLE IX. *Subject C. W. V.*

		Right Eye			*Left Eye*		
		Vertical greater	Equal	Horizontal greater	Vertical greater	Equal	Horizontal greater
Horiz. Standard	1st set of judgments*	39	18	15	17	15	27
	Last set of judgments	56	4	10	26	5	30
Vertical Standard	1st set of judgments*	45	12	8	19	11	30
	Last set of judgments	64	3	4	27	7	24

* With this subject I have taken the first two-thirds of the judgments, and halved the numbers in order to compare them with the numbers of the last third of the judgments. This was unfortunately necessitated by lack of clear distinction between records of the first and second sets of judgments.

With C. W. V. the totals are greater for the right eye, because more variables were used for this eye than for the left.

It will be seen that E. R. V. in the case of Fig. A shows an increase in the illusion for two eyes and the right eye (when vertical is standard), but the illusion for the left eye remains practically the same[1]. It is remarkable that in all cases the "equal" judgments entirely disappear in the last set of judgments. The increase of the illusion in the case of the right eye and two eyes is caused largely by these equal judgments being transferred to the column of "vertical greater" judgments. This disappearance (or decrease) of the number of equal judgments seems to indicate a more definite and decided attitude of judgment in the later sittings[2]. This more decided attitude is shown by all subjects who show an increase in the illusion with practice (E. R. V., W. A., and C. W. V.), and it is remarkable that the opposite effect (*i.e.* an *increase* in the number of "equal" judgments) is shown by the only subject (A. K.) who shows a slight *decrease* in the illusion with practice[3].

In the case of subject C. O. K. the amount of the illusion remained practically the same throughout, and in her case it is notable that from the first her answers were prompt and definite. Throughout a long set of experiments (she had two sets, with and without glasses) she never, even at the start, gave a single "equal" judgment, nor even one in the form "vertical greater or equal" or "horizontal greater or equal."

L. B. also showed no change of the illusion with practice, and no decrease in the number of "equal" and indefinite judgments. But unlike C. O. K., who was prompt and confident from the beginning, L. B. was the most hesitant of all my subjects, and this hesitancy did not decrease with practice in his case. To the end he interjected remarks that it seemed to him that his results could be of little value as it was so hard to form judgments and that these were probably very inconsistent, though as a matter of fact he was as consistent as the average subject. It should be noted that the sittings in the case of L. B. were generally separated by about a week.

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[1] The results are broadly the same for Fig. C, in which again very little increase in illusion was shown by the left eye. Practice effects could not be calculated for Figs. B and D owing to the second reason mentioned above.

[2] This subject explained that by "equal" she meant "I cannot tell the difference." This was the general interpretation, though two subjects asserted that *sometimes* the equal judgments were decided judgments of positive equality, and these two, A. E. T. and C. W. V. were those who had had most experience in introspective work.

[3] This only refers to the right eye. In the first set of judgments with the right eye A. K. gave 9 "equal" judgments, in the last set 20. I should state that this subject's sittings were by no means regular. He did not acquire any information as to the nature of the illusion during the course of the experiments. But it is possible that he began to suspect the presence of some illusion; for example, he volunteered the remark that he seemed to give the judgments "vertical greater" more readily than the judgments "horizontal greater."

VII. *Interpretation of the effects of practice.*

If the illusion be due to physiological causes it seems possible that these various practice effects may be explained as follows. At first the immediate sensory impressions are complicated with and interpreted by experience of the comparative lengths of lines gained in drawing or in seeing objects known to be square, and so forth. Professor Dawes Hicks and Dr Rivers attribute the indefiniteness of long exposure judgments to a similar source[1]. Now it seems possible that with practice the subjects may yield themselves and their judgments more and more to the immediate sensory impressions made by the lines, thus throwing off the correcting influences of experience. Hence only after practice would the physiological conditions have their full original effect. Professor Stout suggests to me, as an interesting case analogous to this, the regaining on the part of the artist of what Ruskin calls the "innocence of the eye," when he learns to see things "as they really are."

No doubt it is conceivable that a psychological cause might also be more effective after similar practice. But it seems more likely that psychological complications should lose rather than gain influence as judgments become more mechanical. This is true of mental processes in general, and it has been found that in the case of several *psychological* illusions practice causes the illusion to *decrease* markedly or even to disappear entirely. At least it seems clear from the above results that those psychological factors which lead to hesitancy and uncertainty tend to disappear with practice[2].

It will be noticed (Table VII) that practice had a much less pronounced effect on the left eye of E. R. V. than on the right eye. Now this left eye is slightly the more astigmatic, and E. R. V. remarked that it was harder to judge with it and that she became more easily fatigued with its use than with the right eye. Possibly then the 'mechanical' attitude was not so easily acquired, though the final

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[1] *Op. cit.* 257.

[2] It is interesting to note that the right eye of E. R. V. also showed a steady increase with practice of the illusion of the over-estimation of the upper half of a vertical line compared with the lower half. Adding results of both standards we get the following:

	Number of times Upper half judged greater	Number of times Lower half judged greater
1st group of 7 judgments ...	30	41
2nd ,, ,, ...	38	36
3rd ,, ,, ...	41	31

disappearance of the "equal" judgments suggests that it *was* acquired eventually.

One further speculation I suggest in the most tentative manner. It is remarkable that, both with E. R. V. and C. W. V. it is *the more efficient eye* which shows the greater increase of the illusion with practice. Is it possible that in ordinary estimations of shape and size we rely mainly upon the impressions received through the better eye, when one is superior to the other? The writer is only slightly more myopic in the left eye than in the right, but he has found a constant tendency to close the left eye (when without glasses), and to rely upon the right only. In such subjects, when the differences are not compensated by glasses, is it not possible that through one eye being habitually more used, that eye alone becomes fully "trained[1]"? Thus the primary physiological illusion might be (partially) corrected by experience only or mainly in one eye, viz. the more efficient one. If this were so the less efficient eye, in such experiments as these, might show its full original degree of illusion at the start. It would have little or nothing to "unlearn." Hence there would only be an *increase* of the illusion with practice, in the case of the more efficient "trained" eye (as we find is the case with E. R. V. and C. W. V.), an increase which would occur in so far as these effects of training and experience were lost in the more mechanical attitude of the subject. If this could be established it would throw an interesting light on the relationship of the respective (or combined) contributions of the two eyes to the processes of association, perception and interpretation.

In view of the effect of practice on the amount of the illusion the following question arises. If the increase of the illusion with practice is associated with increased decisiveness of judgment, is it possible that the differences between the two eyes are similarly causally related to the latter and are not due to physiological conditions? It seems to me that the facts are against this.

For there is no regularity of connexion between (1) size of illusion in respective eyes and (2) efficiency of the respective eyes, or (3) decisiveness of judgment with respective eyes.

Thus in the case of subject A. E. T., (1) the illusion is much greater, seen with the right eye; but (2) the left eye is slightly the more efficient, from which one would expect that certainty should be greater; while (3) the number of indefinite judgments is practically equal for the two eyes.

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[1] It is of course known that when one eye is markedly defective the visual impression received by means of it may be ignored entirely and so squint encouraged. See C. S. Myers, *Text Book of Experimental Psychology*, 1911, I. 266. Cf. also A. A. Bradburne, *Ophthalmology*, July, 1909.

On the other hand, in the case of C. W. V., (1) the illusion is much greater when seen with the right eye; (2) the efficiency of the two eyes was presumably the same, as glasses were worn during experiments; and (3) the number of indefinite judgments was practically the same (proportionately) for each eye.

Or to put it briefly, with these two subjects the illusion is twice or three times as great with the right eye as with the left, and yet the number of indefinite judgments is the same for both eyes.

Lastly, in subject E. R. V., (1) the illusion is considerably greater in the left eye; (2) the right eye has slight superior efficiency; but (3) the number of equal (indefinite) judgments is almost the same (left eye 129, right eye 140).

These facts, however, do not constitute a difficulty for a *physiological* theory, since the illusion may be much greater to start with in one eye than in the other. Changes due to practice (viz. increase of illusion with greater definiteness of judgment) may affect the right eye more than the left, and yet the left may show a greater illusion; or practice may affect the left more, and the left eye may show a greater illusion; or practice may affect the right eye more and the right eye may show a greater illusion and so on, and these are just the various combinations of results which we *do* obtain.

VIII. *The physiological factor involved in the illusion.*

The exact nature of the physiological factor involved in the horizontal-vertical illusion is not primarily a question for the psychologist. We may be content with the supposition that those conditions, whatever they may be, which give rise to variations in the apparent size of objects as seen by different parts of the periphery of the retina, differ from the conditions at the centre more in the vertical direction than they do in the horizontal direction. Poschoga found that the threshold of discrimination of two points is much higher in the periphery than near the fovea, and that the difference increases as we get further from the fovea. This might seem to be readily explained by the fact that the retinal elements are wider apart at the periphery than near the centre. But we have to correlate these facts with the fact that objects look *larger* in peripheral than in macular vision. Now this is quite the opposite to what we find as regards the sense of touch. In the case of touch an object or a line appears largest when in contact with that region of the skin where the threshold of discrimination is *lowest*. There is, however, no direct proportion between the threshold of discrimination and impressions of size. And for various reasons it would seem that the threshold of discrimination is not to be taken as the unit of measurement of distance either in sight or touch[1].

Certainly as regards vision we may conclude that the unit of size cannot be given merely by the stimulation of one retinal element, at least that this is not a *constant* unit for all parts of the retina. But the distance between the sensitive elements in the periphery may in some way give rise to an impression of greater size more than sufficient to compensate for the fact that fewer elements are stimulated by any given object at the periphery than near the centre.

Or again, we know that we may get "a difference of perceived extent by the stimulation *at various intensities*, of a single pressure spot, and of a single retinal cone[2]." If some cones are more sensitive than others, is it not possible that some of their superior activity is translated into an experience of increased extensity beyond that of others? It has been suggested that, as most of our sensations important for life and well-being come from stimuli below the horizontal plane of vision, we may expect the upper half of the retina to be more sensitive (or our preception by means of it more accurate) than the lower. But greater accuracy does not necessarily imply greater size: and further, this theory would not explain the illusion in the figures ┘ and └ when the point of junction is fixated; for the vertical would in this case stimulate the *lower* part of the retinal field.

Of course it still remains possible that the same unit of size has come to mean more, and appear more, in the vertical than in the horizontal meridian. Some obscure psychological factor may be at work here. For example, it is possibly harder to attend to objects in the vertical than in the horizontal periphery, owing to our greater practice with the widely extended horizontal field. And it is conceivable that greater difficulty of attention should be translated into greater apparent size, though one would think that the opposite would be more likely. But we must recall the facts that the illusion may be very different in amount for the two eyes respectively, and that this difference may be as great as or even greater than the whole amount of illusion for one of the eyes. Such facts must surely be explained by a purely physiological factor. They cannot be explained merely by supposing that the more efficient eye has had better training in drawing, observation of square objects, etc., and so gives a smaller illusion. For in some cases it is the *better* eye which gives the greater amount of illusion, markedly so in the writer's own case. On the whole then the simplest explanation seems to be that there are original quantitative differences in the sensations mediated by different meridians of the retina, a relative magnification generally taking place in the vertical periphery. This view we find confirmed by the changes in the relative apparent sizes of a disc or line seen in different portions of the peripheral field of vision, and by the fact that the horizontal-vertical illusion may be present in different degrees in different quadrants of the visual field, even for the same eye.

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[1] See Poschoga, *op. cit.* 427. Also Titchener, *Textbook of Psychology*, II. 322 and 327.

[2] Titchener, *Textbook of Psychology*, II. 327.

IX. *Summary of results and conclusions.*

(1) It appears that the horizontal-vertical illusion is unaffected by astigmatism not exceeding ± 1·5 D, unless other optical defects also cause a blurring of the figure.

(2) But astigmatism of ± 2 D, in some subjects at least, is enough to alter the apparent relative lengths of the lines in the horizontal-vertical figure.

(3) Subjects entirely free from astigmatism, or whose astigmatism is so slight as to be negligible, still show a marked difference between the amount of the illusion for the right eye and that for the left eye. This must apparently be ascribed to some physiological cause.

(4) As this difference between the two eyes is sometimes greater than the whole amount of illusion for one of the eyes, it seems possible that this physiological factor may be the sole or at least the main cause of the illusion.

(5) Some clue as to the locus of this physiological factor may be afforded by the fact that an object appears of different sizes according to the part of the peripheral field of the retina on which its image falls.

(6) The physiological theory here suggested accounts for the apparent variation of the comparative lengths of the lines as one fixates different points of the figure.

(7) Apparently the usual effect of practice is to increase the illusion. The increase may be much greater in the case of one eye

than the other. It is generally accompanied by a great decrease in the number of judgments that the horizontal and vertical lines are of equal length. In the two cases in which the practice effect was greater with one eye than the other, it was the more efficient eye that showed the greater increase of the illusion with practice.

(8) This increase of the illusion with practice seems to be due to the adoption of a more 'mechanical' attitude on the part of the subject, in which he yields himself more completely to the immediate sensory impressions.

I wish to tender my thanks to Dr Rivers for his continued interest in this research, and to the subjects who so kindly submitted to the numerous tests, viz. Professor A. E. Taylor, Dr J. Scott, Professor L. Brehaut and my wife. These, together with the subjects of the previous series of experiments, contributed in all over fifteen thousand judgments.

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THE INFLUENCE OF MARGINS ON THE PROCESS OF BISECTION: ADDITIONAL EXPERIMENTS WITH OBSERVATIONS ON THE AFFECTIVE CHARACTER OF THE DETERMINATIONS.

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The objects and methods of the investigation. Results regarding the magnitude and course of the marginal illusion: modifications due to the special mode of presenting distances. The definite character of the transition from 'far' to 'near' judgments. The time taken in the process of judgment. The feelings connected with the various forms of judgment; the course of affective values in connexion with the succession of stages in the series. Judgments of equality and the accompanying feelings.

THE experiments which are described in this paper form a continuation and extension of the investigation regarding the marginal illusion which was reported in a former number of this *Journal*[1]. A different method has been employed in the present work; certain points have been more fully examined, and new observations have been made for which the method now employed gave a favourable opportunity. It was originally intended that the investigation should include other related points, but as the experiments now described form a connected series, it has been decided to report them in the present form. The work was carried on in the summer of 1910 and in the spring of 1911. Observations were contributed on the first occasion by two of those who took part in the work—the subjects S. and F.: each was in turn experimenter and subject and thus was fully acquainted with the object of the experiments. On the second occasion observations were contributed by N. who acted only as subject: the results in this case are free from the disturbing effects which familiarity with the plan of investigation is apt to produce and which are recognisable in certain sections of the work. As will appear subsequently, there are interesting features in the results which appear to be connected with the different attitudes of the subjects towards the work.

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[1] "The Influence of Margins on the Bisection of a Line," 1909, III. 78.

The apparatus employed in the experiments was the *Distanzvariator* of Chaym[1]. In this apparatus—to refer only to the main features—three thin sheets of glass are held side by side vertically in a framework. Each of these is capable of independent movement, the extent of which is measured by means of a scale with vernier. Opaque marks attached to the glass are visible on a ground or field formed by translucent paper which is stretched across the circular opening in the framework, the diameter of the field being 25 cm. Discs of black paper, 2 mm. in diameter, served as marks in the experiments: these were attached to the different glass sheets in such a way that they lay on the horizontal diameter of the field, and thus presented the distances which formed the objects of the judgments. The apparatus was placed at a short distance from the window of a room facing south-east: the horizontal diameter was about 120 cm. from the ground. A screen of black cloth, extending outwards from the border of the field, filled the window on each side and below, and stretched for a considerable distance above. The subject sat in a chair which was fixed in such a position that the distance between the field and the subject's eyes was approximately 1½ metres. On the first occasion, when the work was done in the afternoon, the brightness of the field, though somewhat too great, was satisfactorily constant. On the second occasion, when the experiments were taken in the forenoon, the light was troublesome on certain days owing to rapid alternation of cloud and sunshine. The difficulty was to a considerable extent met by placing in front of the subject at the foot of the apparatus a large sheet of white cardboard towards which the eyes were directed before the field was looked at.

The distance which was to be bisected by the subject—the main distance—was formed by two discs which were 60 mm. distant from each other: the bisection was accomplished by adjustment of the position of a third, or 'middle' disc. The margins, or marginal distances, were formed by the presentation of a fourth disc which was placed at varying distances from one end of the main distance. The following were their magnitudes, measured in millimetres:—

3, 6, 9, 12, 16, 20, 25, 30, 50, 80, 120.

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[1] The apparatus is constructed by Messrs Spindler and Hoyer, Göttingen.

Throughout the experiments the margins lay on the right of the main distance. A repetition of the experiments with the position on the left, seemed unnecessary in view of the results of the earlier investigation, which had shewn that the illusion was the same in character whether the margin lay on the right or the left of the line which was being divided. It will be noted that by the employment of these margins a wide range of variation is secured, extending from one-twentieth of the main distance to twice that distance. In addition to the cases[1], eleven in number, where a margin is present—which will be termed 'marginal' cases—there are 'normal' cases, in which the main distance appeared without any modifying margin and which thus form a control for the other cases. Owing to the limited size of the field, and to the employment of varying margins, it was not practicable to place the main distance in the centre of the field, or to keep it in the same position throughout the experiments. For the cases with margins of 3 to 30 mm., it held a position somewhat to the left, the central point of the main distance being 30 mm. from the centre of the field: for those with margins of 50, 80 and 120 mm. it was moved further to the left, the displacement being increased with each increase of margin: in this way the main distance occupied four positions in the field. In order that the estimations in the marginal cases should be strictly comparable with those of the normal cases, the apparent or subjective middle point of the unmodified or normal line, was determined in each of the four positions already referred to. We have thus a total of fifteen cases, marginal and normal, in which the process of bisection was studied.

The experiments were carried out in general according to the method of minimal changes, though the procedure differed in certain respects from that commonly employed. In all instances the series of changes in the position of the middle disc[2] was directed towards the central point of the main distance, the starting point being a position which lay clearly to the right or to the left. The steps by which the disc was moved altered in magnitude in the course of each series from 1 mm. to 0·5 mm., the change being determined by the judgment of the subject in the following way. The subject was directed to report his judgment by the single word 'far' so long as the middle disc appeared to lie distinctly to the right or left of the centre: the judgment, however, referred not to the position of the disc on one side or the other, but simply to the fact that it was distant from the centre. When the disc appeared to be approaching the central position, the response was 'near': as soon as the first of the 'near' judgments had been given the magnitude of the changes in the position of the middle disc was diminished to 0·5 mm., this magnitude being retained till the termination of the series. When the position of the disc seemed to coincide exactly with the centre the subject gave the report 'equal': when he was uncertain, the report was 'doubtful.' In no instance was a series carried beyond the stage at which the first 'equal' or 'doubtful' judgment was given. Immediately after the completion of an increasing or decreasing series there followed for the same case a series in the opposite direction.

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[1] Where experimental details are dealt with, the term 'case' is employed throughout the paper in the sense which it bears in this passage.

[2] The terms 'increasing' and 'decreasing,' as applied subsequently to these series, refer to the movements of the middle disc from left to right and from right to left, respectively. The zero point of the scale by which the magnitudes in the series are measured is supposed to coincide with the centre of the left terminal disc of the main distance, so that movement towards the right means increase on the scale.

It did not appear advisable to use steps of less than half a millimetre, or to vary the magnitude of the steps otherwise in the course of the experiments. On the other hand, there was alteration from day to day in the starting point of each series: the order of presentation of the increasing and decreasing series was varied: the place of each case, normal and marginal, in the total work was changed as the work progressed. With one exception determinations were made for all the cases on each day; in some instances additional experiments were made with a section of the cases. The days on which determinations were made were usually separated from each other by days which were free from experimental work. During each sitting pauses were introduced in the work at suitable points: where one person was both subject and experimenter, these functions were exercised alternately. Practice was given to each subject before the beginning of the experiments whose results are recorded in the tables.

In Table I are presented the results regarding the magnitude and course of the illusion in the various cases. The results are divided into three sections according to their general character. The first or normal section contains the determinations of the subjective middle point of the normal line in each of the four positions already indicated: the data for the first position are given in the column headed '0—0,' those for the second are given in column '0—50,' and so on. In the second section are given the results for the margins 3 to 25 mm., with which the illusion proved to be relatively strong, while the third section includes the results for the margins with which the illusion was slight or absent; in each case the margin is stated at the head of the column.

TABLE I. *Magnitude of Illusion.*

		I				II							III			
		0—0	0—50	0—80	0—120	3	6	9	12	16	20	25	30	50	80	120
S.	Last Far	30·4	30·3	30·4	30·0	31·3	30·9	31·1	30·9	30·9	30·7	30·7	30·4	30·0	30·1	29·9
	D.+E. av.	30·4	30·5	30·5	30·2	31·2	31·1	31·0	31·0	31·1	31·0	30·6	30·6	30·2	30·3	30·2
	D.E. c.v.	30·4	30·5	30·5	30·3	31·2	31·2	31·0	30·9	31·1	30·9	30·6	30·5	30·3	30·4	30·3
	°/o					2·6	2·3	2·2	2·2	2·4	2·2	0·6	0·6	-1·0	-0·7	0·0
F.	Last Far	30·2	29·7	29·7	29·8	30·7	31·0	31·8	31·8	31·6	31·3	30·9	30·6	30·7	30·2	30·1
	D.+E. av.	30·2	30·3	29·9	30·0	30·9	31·2	31·8	31·8	31·6	31·6	31·1	30·5	30·5	30·5	30·2
	D.E. c.v.	30·3	30·0	29·8	29·8	30·9	31·0	31·6	31·9	31·6	31·6	30·9	30·6	30·4	30·5	30·3
	°/o					2·4	3·2	5·1	5·4	4·7	4·6	2·9	1·1	0·5	2·2	0·9
N.	Last Far	30·7	31·1	31·0	30·6	31·5	31·7	31·9	31·8	31·7	31·5	31·2	30·7	30·8	30·4	30·2
	D.+E. av.	30·9	30·9	31·0	30·5	31·5	31·7	31·7	31·7	31·5	31·3	31·2	30·9	30·8	30·7	30·4
	D.E. c.v.	31·0	31·0	31·0	30·5	31·5	31·8	31·6	31·6	31·5	31·4	31·2	30·9	30·9	30·6	30·5
	°/o					2·1	2·7	2·7	2·8	2·2	1·3	1·1	0·0	0·0	-0·1	0·0

In the horizontal column opposite 'Last Far' are given the average magnitudes at which the judgment 'far' was last made in the series of successive movements of the middle disc. Opposite 'D. + E. av.' are given the averages of the combined magnitudes for the judgments 'equal' and 'doubtful': the next column contains the central values for these judgments. The averages were first calculated separately for the increasing and the decreasing series, but, as the detailed figures showed no points of interest which could not be presented otherwise in more concise form, the data of the two series have been amalgamated to form the values given in the table. The largest number of determinations was made with margins where the illusion was more evident and with the corresponding normal cases: in all instances the average and central value are based on ten or more separate determinations[1]. While the average is based in each instance on the massed results of the two series, the central value has been determined by first obtaining the central value for each series (increasing and decreasing) taken separately[2], and then taking the mean of these two data. The object of this procedure has been to give an opportunity for the representation of characteristics in each series which might be obscured if all the results were grouped together, as they have been for the average.

The curves given in Fig. 1 are based on the percentages which appear in Table I. The continuous line in this and in the subsequent figure represents the values for S., the dotted line those for F., and the interrupted line those for N. These percentages are calculated on the basis of the D. + E. average, the differences between the value in each marginal case and that in the corresponding normal case being given as a percentage of half the main distance, viz. 30 mm.

The significance of the data for the last 'far' judgments will be discussed later on: we shall meanwhile employ them as a third line of evidence in regard to the course of the illusion, in addition to the data given by the D. + E. averages and the central values. It will be seen, then, that the general course of the illusion for the three subjects is the same. After a very rapid increase in magnitude, which reaches its maximum at latest with a margin of 12 mm., the illusion loses its effectiveness and ceases to appear, or is present only to a slight extent beyond the margin of 25 mm. The descent of the curve as will be noted is, on the whole, only a little less rapid than the ascent. It seems fair to look on the irregularities apparent in the curves, e.g. at margin 16 with S. and at margin 80 with F., as accidental features, especially when the differences at these points between the 'last far' results and those for 'doubtful' and 'equal' are taken into consideration.

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[1] The following are the detailed numbers of the separate determinations made in the various cases.—Subject S.: n = 14 for 0—0 and margins 3 to 20; n = 12 for margins 25 to 120; n = 10 for 0—50, 0—80, 0—120. Subject F.: n = 12 for 0—0 and all the marginal cases; n = 10 for 0—50, 0—80, 0—120. Subject N.: n = 20 for 0—0 and margins 3 to 20; n = 14 for all the other cases. It may be mentioned that on the first occasion, when S. and F. were subjects, the experiments were terminated owing to an accident.

[2] Where the number of separate determinations in each series (incr. and decr.) is greater than six, the central value is based on that number whose ratio to the total is nearest to the ratio in the series with six determinations, *i.e.* 1 to 3. The difference between this and the ordinary mode of treatment involves, for the present purposes at least, no disadvantage.

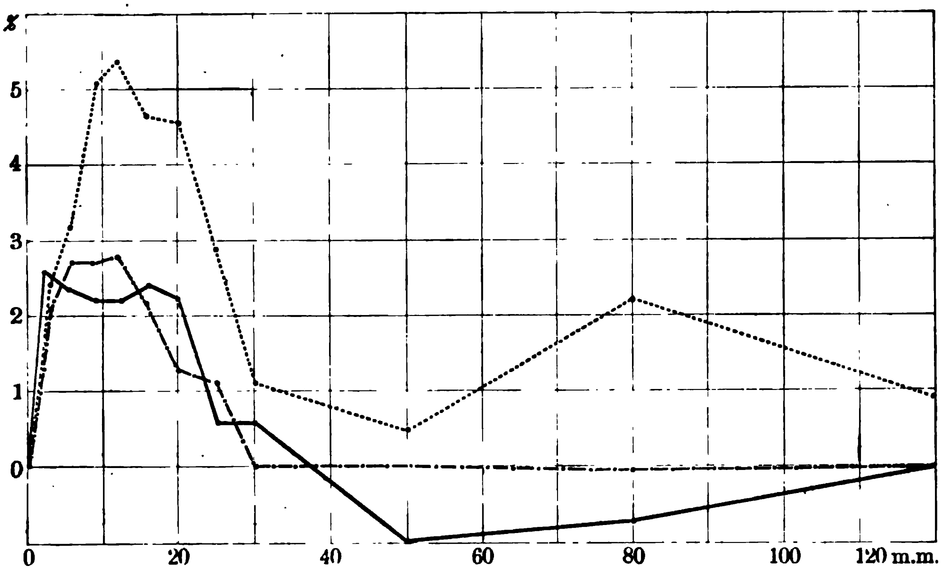

FIG. 1. Magnitude of Illusion.

It may be noted that, apart from the points which will be discussed immediately, the results of the present work confirm the conclusions as to the course of the illusion which were reached on the previous occasion by the employment of the method of reproduction.

In the former investigation the point of maximal effect was found to lie in a region marked by margins whose magnitudes were one-eighth to one-fourth of that of the line which was being bisected. On the present occasion the corresponding region is marked by margins of 3 and 12 mm., i.e., with magnitudes of one-twentieth to one-fifth of the bisected distance. It would appear, therefore, that according to the present results the apex of the curve of illusory effect lies closer to the origin than appeared to be the case on the previous occasion. Divergent results are given also on another point, viz., the fall of the curve after the apex has been reached. In the former investigation the descent was much more gradual than the rise, and, if one may judge from the general tendency of the results, the illusory effect would probably have disappeared with a margin of about twice the length of the divided line. In the present instance the effect may be regarded as practically disappearing with a margin of 30 mm., i.e. of about half the length of the divided line[1]. It is of interest to remark that the difference appears quite plainly in the results of the subject S., who has taken part in both sets of determinations, and whose curve on the former occasion was in close agreement with those of the other subjects.

There are several considerations which may be noted in attempting to explain these points. As regards the position of the apex, it is to be remembered that in the present investigation the examination of the region of maximal effect is more detailed than it was in the former instance and that consequently some part of the change in position might be accounted for as a matter of detail[2]. Again, one may reasonably suppose in view of the general fact of the interaction of spatial contents that the perception of the distances, both main distance and margin, has been modified by the circumstance that the illuminated field is restricted in extent and is surrounded by a dark ground. As an indication of the influence of the surrounding ground there may be mentioned the repeated observation of F. that in the normal case, 0—120, the boundary of the illuminated field was obtrusive and disturbing. The specific direction in which this influence might be expected to shew itself is, however, less clear. The most important consideration would seem to be that a linear distance which is marked only by terminal discs or points is less readily apprehended as a connected unitary whole than one which is presented as a continuous line. When such points are near together the nearness itself supplies a motive to the perception of their connexion: when they are further apart they tend somewhat rapidly to lose connexion and to become for our apprehension mere disconnected points or spots. Keeping in mind the last consideration one can understand why the point of maximal effect produced by the marginal distance should be nearer to the origin of the curve than on the former occasion, the terminal disc appearing rather early as an isolated point. One can also understand why the descent of the curve should be comparatively rapid with increasing distance of the terminal disc from the end of the main distance[1]. Apart from the difference between lines and punctual distances, it may be noted that the extension of the marginal effectiveness on the former occasion was favoured by the circumstance that the total distance, line *plus* margin, coincided with one of the dimensions of the sheet on which the bisecting marks were made. In other words, the character of the material used in the earlier experiments was such as to present directly a connected spatial extent, separated distinctly from the surrounding field.

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[1] It will be observed that while according to the D.+E. average the illusion has the same magnitude with S. at margins 25 and 30, the central value and 'last far' average show that the curve really continued to fall.

[2] Cf. the discussion of a similar point, *op. cit.* p. 90.

It does not seem necessary to discuss at length the general principle on which the results may be explained. The principle of confluence was found adequate to account for the facts of the marginal illusion which were observed in the former investigation, and nothing has been noted in the results of the present experiments which would suggest a modification of this view[2].

In Table II the data regarding the variability of the determinations are given. Six of the cases have been chosen to serve as representatives of the various normal and marginal cases. They will be employed later on in a similar way at other points. It will be noted that 'Last Far' in the first horizontal column for each subject refers, as before, to the determinations in connexion with which the last 'far' judgment was given, and that D. and E. refer to the 'doubtful' and 'equal' judgments. The mean variation was first calculated separately in the various cases for each series (increasing and decreasing) being based on the average magnitude of stimulus for each form of judgment: the mean of the values for the two series was then taken for presentation in the table. As regards the data for D. and E., which are treated separately in this table, the spaces occupied by lines indicate the absence of the corresponding judgment in both series, or the presence of one observation only.

In order that opportunity may be given for comparing the mean variations given in Table II with other forms the following additional

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[1] Reference may be made to the discussion of related points, *op. cit.* p. 92.

[2] According to this view, where the margin is operative the main distance has a certain length subjectively added to it through the action of confluence, so that the position of the middle point appears to be altered.

TABLE II. *Mean Variations.*

		0—0	0—80	6	12	25	80
S.	Last Far	0·5	0·4	0·6	0·7	0·4	0·5
	D.	0·3	0·2	0·4	0·4	0·4	0·3
	E.	0·0	0·3	—	—	0·3	0·3
F.	Last Far	0·5	0·4	0·3	0·4	0·6	0·4
	D.	0·5	0·0	0·3	0·3	0·3	0·4
	E.	0·2	0·4	0·1	0·2	—	0·0
N.	Last Far	0·5	0·5	0·5	0·4	0·3	0·5
	D.	0·1	0·3	0·4	0·2	0·1	0·0
	E.	0·3	0·3	0·3	0·2	0·3	0·4

values have been calculated for the subject F. (*a*) Two sets are formed for each case, one containing all the D. and E. observations in the increasing, the other those in the decreasing series; the *m.v.* is determined on the basis of the average for each set, these two values being then averaged: (*b*) all the observations in both series are regarded as one set and the *m.v.* is determined with reference to the corresponding average—the D. + E. average presented in Table I.

	0—0	0—80	6	12	25	80
(*a*)	0·3	0·3	0·3	0·3	0·5	0·3
(*b*)	0·3	0·3	0·5	0·4	0·5	0·4

These cases being taken as indicating the general features of the determinations, it may be said that the variability does not appear unduly great as tested by any of the procedures.

It is of interest to note that, while the variation of the last 'far' determinations is greater than that of the 'doubtful' and the 'equal' groups, it is by no means excessive, reaching its maximum with S. at 0·7, with F. at 0·6, and with N. at 0·5. It might reasonably have been anticipated that owing to the indefiniteness and uncertainty of the transition from 'far' to 'near,' especially in the cases where the illusion was operative, the magnitudes of stimuli corresponding to the transition would have been affected by a variation so considerable as to render them largely useless for the purposes of calculation. But in addition to the facts regarding the *m.v.*, already referred to, it may be remarked that in all essential points the course of the values obtained from the last 'far' judgments is in accord with that from the D. + E. average, and that a curve representing the former would possess a high degree of regularity.

These facts are interesting from another point of view. One of the subjects, S., felt from the first the difficulty of giving estimations which were free from the influence which previous knowledge of the illusion might exert—whether in the way of favouring, or of opposing the illusion. It was consequently a relief to him when the thought presented itself that a supplementary and helpful line of evidence might in case of need be supplied by the last 'far' determinations, in which this difficulty was in fact never felt. The presence of the marginal distance was disturbing and troublesome to F. also, particularly in the cases with margins of 3 and 6 mm. With this subject however the difficulty was caused, not by the thought of bias, but by the distraction which was occasioned when an additional disc lay close to the termination of the main distance; and the relief which was experienced in the normal cases was due to the simplification of the conditions under which the judgments were made. It will be seen that with all the subjects the experiments might have terminated in each series at the last 'far' judgment—or rather at the first 'near' one—without any material diminution in the value of the results so far as our knowledge of the course of the illusion itself is concerned. When we turn our attention to the bearing of this class of judgments on the bisection of the unmodified line the interesting fact emerges that the middle point as determined through the last 'far' judgments is nearer to the objective centre more often than it is when determined by means of the D. and E. judgments[1]. It may be pointed out finally that there is considerable constancy in what may be termed the range of these judgments, that is, the difference between the average magnitudes given by the increasing and by the decreasing series. The results for each case, instead of being separately stated, have been summarised for each of the Sections (I, II, III), previously indicated. The differences in the values would seem to be significant in relation rather to the individual attitudes of the subjects than to the course of the illusion.

	I	II	III
S.	5·0	4·9	4·8
F.	4·4	4·0	3·9
N.	3·5	3·9	4·1

Reference may now be made to several points in which the individual attitudes of the subjects play an important part. As regards the relative proportions of D. and E. judgments it is found that in the whole of the work the number of D. judgments is approximately 77 °/₀ with S., while with F. it is 60 °/₀, and with N. it is 25 °/₀. These data may fairly be connected with the general relation of the subjects to the investigation. Since N. takes part in the experimental work only in the capacity of subject and is unacquainted with its purpose, he will inevitably tend, apart altogether from any inherent mental characteristics, to answer in what we may call a natural decided manner, by giving the judgment of 'equal' most frequently when the middle disc ceases to be 'near.' The other two subjects on the other hand may be expected to approach the critical point with a certain hesitation which will tend to reduce the number of occasions on which a positive judgment of equality appears, and thus increase the proportion of D. judgments. The fact that the proportion of these judgments is highest with S. finds its explanation at once in the difficulty experienced by this subject when the critical region was reached. Taking the work as a whole we find that a series (increasing or decreasing) contains on the average 6·9 stages (or judgments) with S. and F. and 6·4 with N. With S. there are 3·0 'far' and 2·9 'near' judgments, while with F. and N. the numbers are, respectively, 3·6, 2·3 and 3·5, 1·9.

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[1] For the three subjects and for the four normal cases the last 'far' av. is in respect to the D.+E. av. and to the D. E. c.v. *nearer* in 9 instances, *the same* in 7 instances and *further off* in 8 instances.

The results are somewhat indefinite in their bearing on the question whether there are characteristic differences between the judgments of 'equal' and 'doubtful' in their relation to the series of spatial magnitudes. The question has been examined by a consideration of the frequency with which in the fifteen cases, normal and marginal, the average magnitudes corresponding to 'equal' are greater, or less, than those corresponding to 'doubtful,' the increasing and decreasing series being for this purpose treated separately. With subject N. there is no appreciable difference between the two sets of data. The same may on the whole be said of F., for it is only in the increasing series that a difference becomes perceptible, the average magnitude for 'equal' being more often the greater of the two. With S. a distinct tendency makes its appearance. Those cases being disregarded where the averages are equal in magnitude, or where one of the two forms of judgments is unrepresented, it is found that the averages for 'doubtful' extend further in the direction in which the series is changing, than those for 'equal' in the ratio 9 to 1, both series (increasing and decreasing) giving the same ratio. In other words, the judgment 'doubtful' tends to appear at the termination of a comparatively prolonged series of stages. Certain introspective observations of the subject seem to have a bearing on this point. Occasionally in the course of a series, after a 'near' judgment had been given, there appeared at the next exposure the thought that the disc was now beyond the middle point, this being accompanied by the reflection that owing to the general character of the series the first idea was probably erroneous. The response 'doubtful' was the natural expression of this state of mind.

Observations were made regarding the time taken in the formation of the various judgments which were employed. No constant or significant relation was found, however, between the results and the presence of the marginal illusion. It is obvious that the forms of judgment employed in the present work are different in certain respects from judgments such as 'like' and 'different' which have been employed in other investigations in which the facts of duration were studied: at the same time there appears to be a fundamental identity of character in the processes which permits of a comparison. It may be said then that these observations regarding the time required for the judgment are in agreement with the general conclusions of previous investigators as regards the increase of duration with lessening difference, the increased duration of the 'equal' judgment as compared with the judgment of difference in the 'far' and the 'near' groups, and the still longer time involved in the 'doubtful' judgment[1].

The subjects were directed to report immediately after each judgment the character of the feeling which was aroused in the course of the formation of the judgment. The report was given simply in all instances by the terms 'pleasant,' 'unpleasant,' 'neutral.' There was no intention of suggesting that other forms of feeling were not to be found; the aim was to direct attention to the question of the presence of those kinds which are commonly recognised. It is evident that other factors besides those immediately involved in the formation of the judgment may have contributed to modify the feeling. But our discussion will be directed principally to the general relation between feeling and the succession of judgments in a series, and these factors may be disregarded

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[1] The conclusion of F. Angell that 'like' judgments are slower than 'doubtful' judgments is in opposition to the results of Martin and Müller and appears to be based on insufficient data. Frank Angell, "Discrimination of Shades of Gray for different Intervals of time," *Philos. Stud.* 1902, XIX. 18; Martin and Müller, *Zur Analyse der Unterschiedsempfindlichkeit*, 1899, 196; cf. Münsterberg, *Psychol. Rev.* 1894, I. 45; Henmon, *Arch. of Philos. Psychol. &c.* 1906, No. 8.

TABLE III. *Feeling: Arrangement by Groups.*

			I				II							III			
			0—0	0—50	0—80	0—120	3	6	9	12	16	20	25	30	50	80	120
S.	Far	P.	2·4	2·5	2·5	2·2	1·8	2·4	2·3	2·4	2·1	1·9	2·2	2·0	2·2	2·2	2·1
		U.	0·7	0·7	0·4	0·8	0·9	0·6	0·8	0·7	0·7	1·1	0·9	0·9	0·6	0·8	1·0
	Near	P.	1·0	1·3	1·0	1·4	0·8	1·4	1·2	1·1	1·3	1·5	1·3	1·1	1·3	1·3	1·4
		U.	1·9	1·3	2·0	2·0	2·3	1·6	1·6	1·6	1·5	1·3	1·4	1·9	1·8	1·7	1·4
	D.	P.	0·1	0·2	0·3	0·2	0·1	0·1	0·3	0·1	0·1	0·1	—	0·3	0·1	0·1	0·3
		U.	0·7	0·7	0·3	0·6	0·9	0·8	0·6	0·7	0·6	0·8	0·7	0·3	0·4	0·8	0·4
	E.	P.	0·2	0·1	0·4	0·2	—	0·1	0·1	0·1	0·4	0·1	0·3	0·4	0·5	0·2	0·2
		U.	0·0	—	—	—	—	—	—	0·0	—	—	0·0	0·1	—	—	0·0
F.	Far	P.	2·8	3·1	2·6	3·0	2·7	2·7	3·0	3·0	2·9	2·6	3·0	3·0	3·3	2·9	2·9
		U.	0·4	0·4	0·9	0·4	1·0	0·8	0·8	1·0	0·7	0·9	0·6	0·7	0·6	0·7	0·8
	Near	P.	1·1	1·4	1·8	1·3	0·3	0·8	0·8	0·5	1·0	1·1	0·8	1·2	0·8	1·1	1·0
		U.	1·6	1·0	1·0	1·7	1·3	1·7	1·1	1·2	1·2	1·5	1·5	1·4	1·1	1·2	1·2
	D.	P.	—	—	0·1	—	—	0·2	0·1	0·2	0·2	0·2	0·1	—	0·1	0·2	0·0
		U.	0·5	0·5	0·2	0·3	0·8	0·5	0·6	0·6	0·4	0·4	0·8	0·4	0·5	0·4	0·8
	E.	P.	0·4	0·5	0·6	0·6	0·3	0·3	0·2	0·3	0·4	0·3	0·1	0·6	0·4	0·3	0·2
		U.	—	—	0·1	0·1	—	—	0·1	—	—	0·1	0·1	—	0·0	0·1	—
N.	Far	P.	3·8	3·1	3·6	3·5	3·5	3·6	3·3	3·6	3·7	3·6	3·4	3·5	3·4	3·3	3·5
		U.	—	—	—	0·1	0·1	0·1	0·1	0·1	0·1	0·0	—	0·0	—	0·0	0·0
	Near	P.	0·9	0·7	0·8	0·4	0·5	0·7	0·8	0·6	0·7	0·6	0·9	0·6	0·7	0·5	0·9
		U.	1·4	1·4	1·0	1·0	0·1	0·1	1·5	1·0	1·2	1·1	1·7	1·5	1·6	1·5	1·4
	D.	P.	0·1	—	—	—	—	—	0·1	—	—	0·0	—	—	—	—	—
		U.	0·2	0·1	0·3	0·4	0·5	0·3	0·2	0·3	0·2	0·3	0·3	0·1	0·4	0·1	0·1
	E.	P.	0·5	0·6	0·4	0·4	0·3	0·5	0·5	0·6	0·5	0·4	0·4	0·6	0·6	0·6	0·8
		U.	0·3	0·4	0·3	0·3	0·2	0·3	0·3	0·2	0·3	0·3	0·4	0·3	0·1	0·3	0·1

since their influence is general and is not directed to any definite point in the series.

In analysing these observations regarding affective processes, two methods of treatment have been employed: they may be termed the arrangement by groups and the arrangement by stages. According to the arrangement by groups, which will be taken up first, the observations in each of the groups of 'far,' 'near,' 'doubtful' and 'equal' judgments are combined; the mean frequency of 'pleasant' and 'unpleasant' reports is then determined for each case. The 'neutral' reports, which have not been separately presented, are distributed in two ways. According to the first, which is employed in the preparation of Tables III to VI and which is regarded as the more important, these reports have been halved, one half being assigned to the set of 'pleasant' reports (P.) and one to the 'unpleasant' reports (U.). The effect of this assignment on the proportions of P. and U. reports is to render their differences less acute: as will be seen later on, the magnitude of these differences is such that this modification leaves their characteristic features quite clear. The other mode of distribution, which from the statistical point of view has certain advantages, consists in assigning the 'neutral' reports according to the proportions of the P. and U. reports. It will be noted that this distribution is equivalent to disregarding these reports in calculating the proportions. Reference will be made to the results of this mode of assignment at a later point.

TABLE IV. *Feeling: Arrangement by Groups: Summary.*

		S.				F.				N.			
		I	II	III	All	I	II	III	All	I	II	III	All
Far	P.	2·38	2·16	2·11	2·20	2·83	2·83	3·01	2·88	3·50	3·52	3·45	3·49
	U.	0·64	0·81	0·82	0·77	0·49	0·82	0·70	0·70	0·03	0·05	0·03	0·04
Near	P.	1·18	1·22	1·25	1·22	1·38	0·77	1·05	1·01	0·72	0·69	0·66	0·69
	U.	1·81	1·62	1·71	1·70	1·31	1·36	1·22	1·31	1·20	0·96	1·49	1·17
D.	P.	0·19	0·11	0·19	0·15	0·03	0·13	0·08	0·09	0·01	0·01	—	0·01
	U.	0·58	0·72	0·46	0·62	0·41	0·56	0·52	0·51	0·23	0·28	0·18	0·24
E.	P.	0·22	0·16	0·33	0·22	0·52	0·27	0·36	0·36	0·48	0·45	0·62	0·50
	U.	0·01	0·01	0·03	0·02	0·05	0·04	0·03	0·04	0·29	0·27	0·21	0·26

The arrangement by groups has been followed in the preparation of Tables III and IV. It will be observed that this arrangement enables us to see at once not merely the mean frequency of feeling, but also the average number of judgments which each group contains within a series[1]. As regards the last four horizontal columns it is to be noted that, since the 'doubtful' and 'equal' judgments are alternatives, the average values in each column are always fractional. The results are summarised for the three sections in Table IV: the column headed 'All' gives the final mean which is based on the observations in all the fifteen cases irrespective of the division into sections.

It will be observed that the general relations of affective frequency in the different groups are the same for all the subjects. In the 'far' group the 'pleasant' reports far outnumber those which are 'unpleasant': this relation is reversed in the 'near' group, though the predominance of unpleasantness is by no means so great as was that of pleasantness in the former group. The 'equal' judgments are decidedly pleasant: the 'doubtful' judgments are characteristically unpleasant. These relations while appearing most clearly in the summary are found to hold with few exceptions in the detailed results of Table III in spite of considerable differences in the absolute values. Since there is no distinct and regular variation in the values from section to section, we may assume that the presence of the illusion has not produced any marked result. This circumstance enables us to regard the differences in the separate cases as being mainly accidental and thus to attach a higher representative value to the results given in the 'All' column. With respect to individual differences it is clear that the frequency of pleasantness is on the whole greatest with the subject N. and least with S. There is ground for considering that much of this difference is due to the different attitudes towards the work, which have already been referred to. The same conditions which, it was suggested, account for the preponderance of 'doubtful' judgments will tend in general to raise the number of 'unpleasant' judgments. It should be observed also that a sense of strain was frequently prominent with S. in the course of the formation of the judgment, which was in part connected with the knowledge of the subject that measurements of duration were being made: this no doubt contributes in part to the great frequency of unpleasant feeling.

The object of the arrangement by stages was to determine in detail the character of the affective process at successive points or stages in the series. For the purposes of this inquiry the last 'far' judgment with its accompanying feeling has been considered as a fixed point of

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[1] In Table III and elsewhere in the following pages, zero indicates a numerical value less than 0·05, while a line indicates the absence of observations at that point.

TABLE V. *Feeling: Arrangement by Stages: Percentages of Unpleasantness.*

			I				II								III				General
			0—0	0—50	0—80	0—120	3	6	9	12	16	20	25	M.V.	30	50	80	120	M.V.
S.	Far	2	14	10	—	10	25	7	7	—	7	14	8	6	10	8	17	8	4
		3	57	60	40	65	64	50	71	71	64	86	91	10	75	50	58	92	12
	Near	1	85	63	89	89	71	75	62	75	50	68	55	8	80	73	73	67	9
		2	39	10	43	45	56	22	33	48	28	20	8	16	47	50	46	27	13
		3	73	63	78	44	93	71	77	50	75	55	86	10	80	82	64	67	11
F.	Far	2	—	—	5	—	13	13	8	13	4	4	8	3	4	8	5	13	4
		3	42	35	80	30	79	63	71	83	67	83	54	9	67	50	54	67	13
	Near	1	67	43	28	78	86	77	42	67	59	70	89	12	70	67	65	63	12
		2	21	22	10	37	100	40	83	25	—	22	8	30	13	—	13	21	20
		3	79	79	61	67	86	68	92	67	73	80	78	7	65	67	70	88	8
N.	Far	3	—	7	—	11	13	5	5	5	5	3	—	2	4	—	4	4	3
	Near	1	30	36	25	42	50	38	43	40	35	39	36	4	50	42	38	35	5
		2	65	75	67	100	67	75	70	70	69	75	79	4	75	75	72	50	6
		3	90	82	88	100	92	88	87	80	89	94	93	3	91	96	92	80	4

reference, before which come two other judgments, the three being taken together as forming the first part of a regular series, for each stage of which the average frequencies of 'pleasant' and 'unpleasant' feeling are determined[1]. The group of 'near' judgments presents a more complex problem, for, while the number of judgments occasionally exceeds three, more often it falls below it. The following treatment, in which three stages are again distinguished, while in some respects arbitrary, gives an instructive view of this portion of the series. When there are three successive judgments in this group, the frequency of feeling is determined simply as before in each instance. When there are more than three, the first and last are taken as forming the first and third stage: the others are taken together as forming the second or intermediate stage, which counts as one, so that each single affective report which is included has a fractional value, e.g. one-half or one-third. From the sum of such values the affective frequency is then calculated. The results for the successive stages[2] are given in Table V in the form of percentages of unpleasant feeling. In the last vertical column of Section II are given the mean variations for this section as determined on the basis of the values appearing in the table: in the final column under 'General M.V.' are given the corresponding values for all the cases normal and marginal[3]. A summary is given in Table VI: the data for D. and E. in Table III have been transformed into percentages and their average values presented in Table VI together with the previous stages, so that a complete view of the course of affective changes may be obtained.

The percentages of unpleasantness given in the column headed 'All' in Table VI are represented in Fig. 2. A glance at this figure is sufficient to shew that although 'neutral' reports are included each curve has a sufficiently distinctive form.

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[1] Series in which the number of 'far' judgments was less than three have been included, the calculations being correspondingly modified.

[2] The omission of the data for certain stages in Table V is due to the fact that unpleasant feeling rarely occurred at these stages. The statement of the actual values may be given here:—at the first 'far' stage percentages 8 and 11 appear at margins 20 and 80 with S. while percentages 8 and 9 appear at margins 3 and 80 with F.; there are no values for unpleasantness at the first two stages with N.

[3] The values of the M.V. for 'doubtful' and 'equal' are as follows:

	S.		F.		N.	
	II	Gen.	II	Gen.	II	Gen.
D.	8	13	11	13	6	5
E.	8		18	12	5	8

TABLE VI. *Feeling: Arrangement by Stages: Summary.*

		S.				F.				N.			
		I	II	III	All	I	II	III	All	I	II	III	All
Far	1	—	1·2	2·8	1·3	—	1·2	2·3	1·2	—	—	—	—
	2	8·6	9·8	10·8	9·8	1·2	8·9	7·4	6·5	—	—	—	—
	3	55·5	71·1	68·8	66·3	46·7	71·4	59·4	61·6	4·5	5·0	2·7	4·2
Near	1	81·2	65·1	73·0	71·5	53·8	69·9	66·0	64·6	33·3	40·0	41·1	38·5
	2	34·4	30·8	42·4	34·8	22·5	39·8	11·4	27·6	76·6	71·9	68·1	72·1
	3	64·5	72·4	73·0	70·4	71·4	77·5	72·3	74·5	89·8	88·9	91·8	89·9
D.		73·4	87·7	69·7	79·1	91·7	80·2	86·3	84·9	95·0	96·0	100·0	96·8
E.		4·2	6·3	8·4	6·8	7·2	15·4	7·5	11·1	37·1	37·8	24·4	34·0

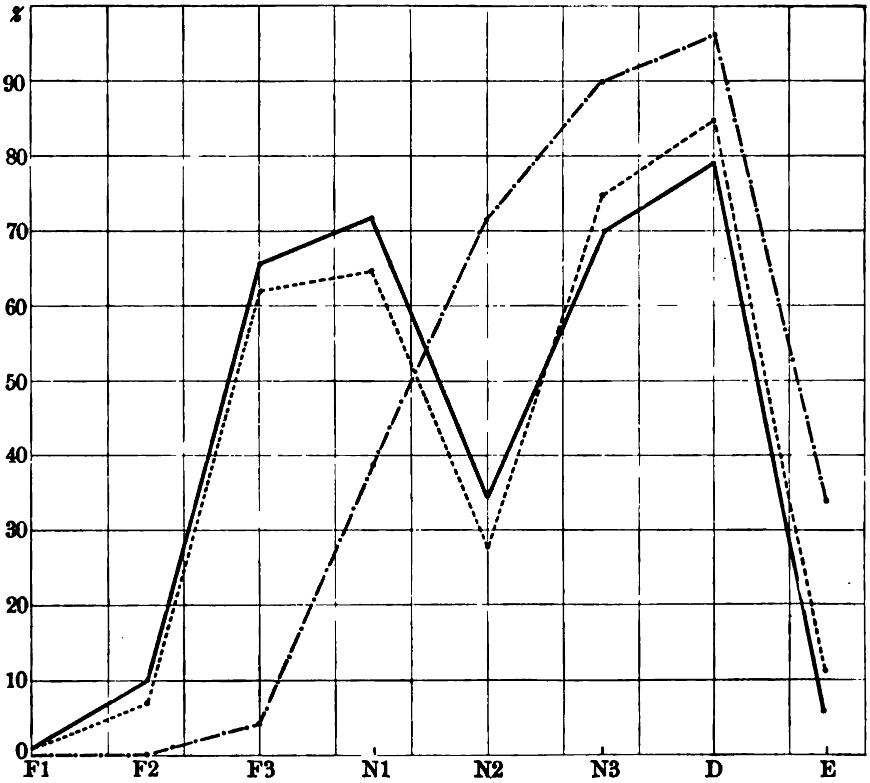

FIG. 2. Feeling connected with Judgment: Arrangement by Stages.

It is not surprising that the values for the stages in the various cases should exhibit great variations. But when these values are considered in connexion with each other in the succession of stages, there appears in the great majority of cases a definite order of values. This order appears with much clearness in the summary and in the figure. The general rule is that the frequency of unpleasant feeling increases at first slowly, then rapidly, then again slowly. It attains its highest point at the stage of 'doubtful' responses, but is strikingly diminished at the stage of equality.

The curve of the subject N. exhibits the greatest regularity and it may perhaps be taken as exemplifying the normal course of affective values: the low value of unpleasantness for S. and F. at the intermediate stage of the 'near' group would then be a deviation due to special conditions. From this point of view the very high frequency of unpleasant feeling with S. and F. at the last 'far' and the first 'near' stage would represent the difficulty which was experienced in the transition from one form of judgment to another. The affective character of the intermediate stage, in which the main feature is the continuation and establishment of the form of judgment already reached, would then represent for these subjects a lessening of the difficulty, heightened perhaps by contrast. A certain relief was noted by S. and F. at this stage altogether apart from any knowledge as to the direction of the numerical results. The fact that the curve of the subject N. does not shew these modifications may be connected with that natural attitude towards the work which has already been referred to. Apart from these special modifications, it may be assumed that the increasing unpleasantness which accompanies the progress of the series finds its main explanation in the lessening clearness of the perception of difference together with the increasing uncertainty and difficulty in the formation of a judgment which are thereby involved.

A special problem is presented by the marked diminution in the frequency of unpleasantness which characterises the judgments of the 'equal' group. There may well have been an aesthetic element in these judgments, but this, though present in certain cases, was not observed in this group and can hardly have been a dominant condition. It may be suggested that a more important factor is to be recognised in the specific character of the judgment of equality. Each subject was left free to answer 'doubtful' should it appear appropriate, and the fact that the number of such responses is considerable shews that this alternative was readily chosen. It may be supposed then that the judgment of equality when it does appear is based on a clear definite apprehension of spatial relation. The attention in this process may be directed primarily to the likeness of the two parts of the divided line or to the central position as such. In either case the process is relatively positive and precise in character, and thus it will naturally be accompanied by feeling which is predominantly pleasant.

It is stated by Whipple[1] that "Pleasantness is the correlate of 'certain' judgments not of any of the categories 'equal,' 'higher,' or 'lower.'" That there is a decided element of certainty in the judgments of equality is no doubt the case, but this element taken by itself does not appear capable of giving an adequate explanation of the character of the feeling in the present instance. The 'near' judgments possess a fair degree of certainty—the 'doubtful' response was in a way an alternative to 'near,' as well as to 'equal'—yet they are distinctly more unpleasant than the 'equal' judgments. It may be pointed out further that certainty is not a primary fact: it is a result which, if accidental conditions such as the mood of the moment be disregarded, must be attributed to the inherent nature of the judgment in regard to which we 'feel' certain. We seem to be justified, then, in going beyond this secondary fact, and in recognising that in addition to certainty the character of the judgment is a condition of feeling. It is possible, indeed, that such a directly originated state of feeling may in some instances contribute to the appearance of the state of certainty.

Angell[2] remarks that "the judgments of 'like' differ from those 'unlike' in being in great part negative. They are, so to speak, a function of the 'unlike' judgments....Accordingly judgments of 'like' result in great part from failure to perceive a difference." Such a position could not be taken with regard to the results of the present investigation. The finding of the central point was the essential problem throughout the work, and the 'near' and 'far' judgments, though in a measure positive—inasmuch as a position which is not central is still a definite object of apprehension—were from the point of view of the present investigation primarily negative. There may be mentioned here the observation of the subject S. that in certain cases where the judgment of equality was given it was based on an immediate apprehension of the presence of the middle disc in the central position, in other words on a 'positive impression' as to position. Such a process might be expected to find a reflexion in the kind and degree of feeling at this stage, and it is worthy of remark that the lowest percentage of unpleasantness is met with in the case of this subject, a percentage which is lower than any that appears subsequently to the first stage of the 'far' group in his series.

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[1] "An Analytic Study of the Memory Image and the Process of Judgment in the Discrimination of Clangs and Tones," *Amer. J. of Psychol.* 1901, XII. 445.

[2] *Op. cit.* 19.

The question may be raised whether there is evidence of alteration in the character of feeling in the course of the work. The six representative cases have been analysed from this point of view and the following results have been obtained. The numerical values are percentages of 'pleasant' reports: the terms 'normal' and 'marginal' refer to the cases studied: the upper and lower sets of values, (*a*) and (*b*), refer to the results of the first two days of work and the last two days of work respectively.

	S.		F.		N.	
	Normal	Marginal	Normal	Marginal	Normal	Marginal
(*a*)	50	59	71	66	70	58
(*b*)	59	52	64	67	72	74

With the subject N. there appears a tendency towards increase in the frequency of pleasantness: with F. the tendency is reversed: with S. there is no evidence of any significant alteration.

We may now return to the consideration of the 'neutral' reports. A direct estimate of the extent to which they enter into the affective totals has been gained by analysis of the data in the six representative cases; the average numbers of these reports in the different groups of judgments in a series are given below. An indirect view of their influence is obtained by comparison of the values in Table VI with those in Table VII.

	Far	Near	D.	E.
S.	—	0·1	—	0
F.	0·2	0·2	0	0
N.	0·1	1·1	0	0·4

In the preparation of Table VII the second mode of treatment has been applied to the 'neutral' reports. In dealing with the intermediate stage of the 'near' group a further modification has been introduced. The same data have been employed as before: in this instance, however, whether one, two, or more observations are included in this stage, each counts as one in the formation of the P. and U. totals which are employed in calculating the relative percentages.

TABLE VII. *Feeling: Arrangement by Stages: Summary.*

		S.				F.				N.			
		I	II	III	All	I	II	III	All	I	II	III	All
Far	1	—	1·2	2·8	1·3	—	1·2	2·3	1·2	—	—	—	—
	2	8·6	9·6	10·8	9·6	—	5·1	4·4	3·5	—	—	—	—
	3	56·0	71·1	68·8	66·4	45·0	74·1	61·1	62·9	1·8	0·7	—	0·8
Near	1	81·2	66·1	73·0	72·0	53·1	70·2	66·5	64·6	3·1	15·5	22·9	14·2
	2	36·9	31·4	42·4	35·5	12·7	44·7	10·1	26·9	100·0	100·0	100·0	100·0
	3	64·5	73·1	73·0	70·9	75·8	79·0	73·9	76·8	100·0	100·0	100·0	100·0
D.		73·4	87·9	69·7	79·2	91·7	81·2	88·7	85·8	95·0	100·0	100·0	98·7
E.		—	—	4·2	1·1	3·6	14·8	5·0	9·2	20·4	8·7	16·3	13·9

It has not seemed necessary to present the detailed data for the various cases: an adequate conception of the alterations produced by this treatment is gained by considering the summary given in Table VII in relation with that given in Table VI. It may be said that while certain points in regard to the affective values with each subject are brought out with special clearness by the second treatment, it does not appear that any modifications are required in the conclusions which were based on the results given in the previous tables.

SUMMARY.

1. While the general conclusions of the former investigation regarding the marginal illusion are confirmed by the present work, the influence of the margin has been found to be more restricted, the illusion practically disappearing with a margin which is approximately half the length of the bisected line; at the same time the maximal effect has been found to be produced by a shorter margin. It is considered that these features are connected with the mode in which the distances are presented on the present occasion.

2. The different forms of judgment have quite definite affective values. The frequency of unpleasant feeling is slight with the 'far' judgment, but rises very high with the 'near' judgment: the 'doubtful' judgment is characteristically unpleasant while the 'equal' judgment is pleasant. When the successive stages within the groups of 'far' and 'near' judgments are considered, it is found that on the whole unpleasant feeling increases in frequency, the increase being at first slow, then rapid, then again slow.

(*Manuscript received* 16 *August*, 1912.)

PUBLICATIONS RECENTLY RECEIVED.

The Montessori Method: scientific pedagogy as applied to child education in "The Children's Houses," with additions and revisions by the author. By Dr MARIA MONTESSORI: translated by ANNE E. GEORGE, with an introduction by Professor HENRY W. HOLMES. Pp. xlii + 377. London: Heinemann, 1912.

We must be content to allow such an enthusiast as the Dottoressa to tell her story in her own way, and curb our impatience to get at the Method, as we read through various chapters devoted to such general problems as the new pedagogy, the history of methods, discipline, or the child's diet, and repeated descriptions illustrating the ecstatic delight of her pupils, and her profound faith in her work.

The Montessori Method has an interesting origin. It arose by adapting to the education of normal children methods and apparatus which had been successfully employed to teach deficient children. These methods and apparatus were the outcome of many years of experimental work, based on the principle of maximal freedom for school children, and individual, sensory, motor and intellectual training.

Psychology: a new system, based on the study of the fundamental processes of the human mind. By Dr ARTHUR LYNCH. Vol. I. pp. xxiv + 378, vol. II. pp. xv + 436. London: Swift & Co. Ltd, 1912.

Probably the following quotations will serve to illustrate the original character of this work: "...let us speak of Idea as the image which the mind displays of a past experience, such as a particular sensation" (p. 28). "The fact that we can speak of two shades of red, as being both red...implies something common in the midst of diversity. That something common corresponds to the feeling of innervation of the nervous tissue corresponding. That feeling we may call, by definition, the Feeling of Effort" (pp. 707, 708). Even the index is remarkable for its originality, containing as it does many such entries as "Fielding was gouty [page] 627"; "Mirabeau has some likeness to Gambetta [page] 643"; "Muensterberg has studied legerdemain [page] 42."

Die Entwicklung des Naturgefühls und *Das Wesen der Inspiration.* Von Dr R. HENNIG. S. 160. Leipzig: Johann Ambrosius Barth, 1912. Mk 5.

These two papers, originally read before the Berlin Psychological Society, contain an interesting collection of examples, illustrating the varying degree of appreciation of natural scenery in different ages, and the manner in which, from their own accounts, men of genius arrive at their discoveries.

Fortschritte der Psychologie und ihrer Anwendungen. Unter Mitwirkung von Privatdozent Dr· **Wilhelm Peters**, herausgegeben von Dr **Karl Marbe**. Band I, Heft 1, pp. 82. Leipzig: B. G. Teuber, 20 Juni 1912. Der Band Mk 12.

This journal, which is to appear at irregular intervals—six numbers making up a volume—promises to be of interest not only to psychologists but to those working in various other fields of science. In this first number the editor, Dr Karl Marbe, gives an excellent review of recent work in the application of psychology to natural science, medicine, philology, literature, aesthetics, history, pedagogy, jurisprudence, political economy and philosophy.

Tierpsychologisches Praktikum: in Dialogform. Von Prof. **Karl Camillo Schneider.** S. 719. Leipzig: Veit & Co. Mk 16; geb. Mk 17.50.

A text book in the form of a controversy between a psychologist, a physiologist, a biologist, a vitalist, a "Darwinist," a "Lamarckist" and a monist.

Essentials of Psychology. By Prof. S. **Radhakrishnan.** Pp. 75. Oxford University Press: Henry Frowde, 1912. 1*s.* 4*d.* net.

This is a publication of a short series of lectures delivered by the author in an Indian University. What merit the book has is largely dependent on quotations from Ward, Stout and James, whose writings are liberally drawn upon.

The Outlines of Educational Psychology. By Dr W. H. **Pyle.** Pp. x + 254. Baltimore: Warwick & York, 1911. $1 25 c.

La Religion de l'Evolutionisme. Par **Willem Vogel.** Pp. 334. Bruxelles: Fischlin.

An Introduction to Psychology. By Prof. **Wilhelm Wundt**: translated by Dr **Rudolf Pintner.** Pp. xii + 198. London: Allen & Co., 1912. 3*s.* 6*d.* net.

Studies in Linguistic Psychology. By Prof. **Robert James Kellog.** Pp. 64. Vol. I. No. 1. Illinois: University Press, 1912.

Das Problem der Objektiven Möglichkeit. Von **August Gallinger.** S. viii + 126. Leipzig: Barth, 1912. Mk 4.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

July 13th, 1912. The Heredity of Colour-blindness and its Bearing on the Classification of Cases (Demonstration), by S. P. HAYES (introduced by C. S. MYERS).

A New Apparatus for determining Retinal Sensibility (Demonstration), by C. S. MYERS.

The Evolution of "Psychical Distance," by E. BULLOUGH.

Experiments on Rhythm (Demonstration), by L. WITTGENSTEIN and B. MUSCIO (introduced by C. S. MYERS).

An Experimental Investigation of Perception, by F. SMITH (introduced by C. S. MYERS).

VOLUME V MARCH, 1913 PART 4

THE INFLUENCE OF ATTENTION IN ILLUSIONS OF REVERSIBLE PERSPECTIVE.

BY J. C. FLÜGEL.

[*From the Psychological Laboratory, University College, University of London.*]

I. Introduction. Nature of the Investigation.
II. Experiments with two reversible figures seen simultaneously.
III. Experiments with a series of drawings designed to make the appearance of one perspective progressively more difficult.
IV. Experiments with different Attitudes. Importance of the direction of the Attention. Individual differences.
V. Experiments on Fixation.
VI. Further observations on the influence of Attention. General Conclusions. Reasons for the neglect of Attention in previous investigations.
VII. Some problems arising in connexion with the present results. Summary.

I.

THE interest which experimental psychologists have for long displayed in the subject of illusions of reversible perspective has of late years shown no signs of diminishing. But though the work of recent authors has brought to light a considerable number of new facts and given rise to interesting discussions of old problems from new points of view, there exists as yet no general agreement as to the conditions which underlie the reversals of perspective, or as to the exact nature of the psychological processes involved.

The explanations hitherto advanced may be roughly classified into two main groups, according as they regard the essential conditions of reversal to be found in (1) peripheral, or (2) central factors. Among earlier investigators of note, who have held the view that the changes of perspective depend primarily upon changes in the sense organ, may be mentioned Brewster, Necker and Wundt. Of these Wundt has probably exercised a greater influence on subsequent thought than any other writer on the subject. Among those who have attributed most importance to changes of central origin the best known are perhaps Wheatstone, Helmholtz and Hering.

Until a few years ago the former school was probably in the ascendant, but the most recent writers on the subject have generally expressed dissatisfaction with the view that the reversals are determined solely or principally by peripheral factors, and are inclined to seek an explanation in terms of purely psychological processes, or, at least, of physiological processes affecting the brain rather than the eye.

Thus Becher[1] disputes the validity of Wundt's rule according to which the nature of the perspective seen in an ambiguous figure depends solely upon the direction of fixation or upon eye movements. He comes to the conclusion that at least three factors must be taken into account, viz. fixation (in Wundt's sense), the direction and distribution of attention and 'set' (as manifested in will, preperception, etc.). Benussi[2], starting from observations showing that the time required for an ambiguous figure to be seen in any perspective at all varies according to the position in which the figure is drawn, argues the importance (though not the exclusive importance) of associative factors, and in an interesting theoretical discussion brings the reversals of perspective into relation with the concept of 'shape quality' (*Gestaltqualität*).

De Boer[3], in a recent paper, upholds the importance of associative as against peripheral factors. Wallin also, who in his earlier book[4] was a staunch supporter of the peripheral theory, has, in a later paper[5], suggested an explanation in terms of the 'refractory period of the nervous arc' demonstrated by Sherrington and others.

Those who have endeavoured similarly to account for the reversals of perspective by means of corresponding cerebral changes have, for the most part, brought the perspective illusions into relation with those other cases of alternating mental contents frequently classed together as due to 'fluctuations of attention'; they have in fact regarded the illusions of reversible perspective as constituting only a particular case of a more general mental characteristic, which in turn is due to some general condition affecting the activity of the cerebrum. Slaughter[1], Bonser[2] and others have produced some interesting experimental evidence that this condition is to be found in vaso-motor changes, and more especially in the Traube-Hering waves.

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[1] "Über umkehrbare Zeichnungen," *Arch. f. d. ges. Psychol.* 1910, XVI. 397.

[2] "Über die Motiven d. scheinbaren Körperlichkeit bei umkehrbaren Zeichnungen," *Arch. f. d. ges. Psychol.* 1911, XX. 363.

[3] "Über umkehrbare Zeichnungen," *Arch. f. d. ges. Psychol.* 1910, XVIII. 179.

[4] *Illusions of Reversible Perspective*, Princeton, 1905.

[5] "The Duration of Attention, Reversible Perspectives, and the Refractory Phase of the Nervous Arc," *J. of Philos., Psychol., etc.* 1910, VII. No. 2.

Approaching the problem from a rather different point of view, McDougall[3] has maintained that the illusions we are here considering represent only a special case of a very general psychical characteristic, viz. the rapid fatigability and general instability manifested in all the higher levels of consciousness, and has shown grounds for believing that the physiological basis of this characteristic is to be found in the rapid changes of resistance at the synapses brought about by any nervous discharge affecting the higher centres.

The present investigation was started in the hope of bringing some further light to bear on the problem of the intimate nature of the psychological processes concerned in these illusions, by carrying out some observations which, to the best of the writer's knowledge, had not been made before, or at least had not been made the object of systematic experimentation. The research owes its origin to Prof. Carveth Read, who suggested the line of investigation to be adopted and who collaborated with the writer in the earlier part of the experimental work. Prof. C. Spearman has afforded much valuable help and criticism, both as regards devising and carrying out the experiments and the interpretation of the results obtained. The writer's thanks are also due to those who have at one time or another submitted to the often rather tedious experiments with much patience and goodwill. The observers throughout the investigation were all trained subjects, being teachers, researchers, or advanced students of psychology. Great importance was attached to systematic introspection; and as all the observers were given a very considerable amount of practice with the reversible illusions that were used, and as the reports of their experiences under similar experimental conditions remained on the whole remarkably consistent even throughout long series of experiments, considerable reliance may be placed on the results thus obtained.

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[1] "The Fluctuations of Attention in some of their Psychological Relations," *Amer. J. of Psychol.* 1901, XII. 913.

[2] "A Study of the Relations between Mental Activity and the Circulation of the Blood," *Psychol. Rev.* 1903, X. 120.

[3] "The Physiological Factors of the Attention Process," *Mind*, 1902, XI. 316; 1903 XII. 289, 473; 1906, XV. 329. Especially XV. 340 ff.

The principal apparatus used in all the experiments consisted of a triple time marker writing on a smoked drum and a Jacquet chronograph, adjusted to record seconds, electrically connected with one of the levers of the time marker. The subject recorded his experiences by means of a two-way switch, making contact on the right side during the appearance of one perspective and on the left side during the appearance of the other. The right and left contacts were connected with different levers of the time marker. When neither perspective was seen or when the subject was in any way uncertain as to the nature of the perspective, he refrained from making contact in either direction. Each subject was allowed to choose for himself which contact should correspond to which perspective, and was allowed to practise the method of recording whenever necessary. Each experiment was timed to last one minute. In some cases it was difficult to obtain a typical record in a shorter time than this, while with longer periods the attention of the subject was apt to flag and subsequent introspection became difficult and unreliable.

II.

The first experiments to be described consisted in observing the changes of perspective that occurred in two ambiguous figures presented simultaneously to the subject. It was considered that such experiments, by showing whether the perspective of the two figures tended to change together or independently of one another, would be likely to throw some light on the nature of the processes involved, and would in any case be of interest in view of some of the explanations that have been offered. Thus, if the reversals depend primarily upon some physiological factor which must affect the whole of the cortex simultaneously or nearly simultaneously, we should expect that the changes of perspective would take place in both figures at approximately the same moment. A similar correspondence in the behaviour of the two figures might be looked for, if, as Wundt's results tended to indicate, the reversals are dependent upon some one peripheral factor such as fixation, though the changes in any particular case would depend on the figures used and the direction and extent of the eye movement. If, on the other hand, the fluctuations in the two figures take place with any considerable degree of independence, it would seem that they must depend at least in part upon strictly localised conditions, though this would not of course exclude the partial operation of a more general factor also.

Three sets of experiments were carried out in this connexion. In each case the two figures chosen for use were those of a prism and a table as depicted in Figs. 1 and 2. In the first set of experiments two *identical* figures (*i.e.* two drawings of the same figure, prism or table as the case might be) were placed alongside one another and the subject fixated a point between them. The drawings were of such a size and placed at such a distance from the subject that the whole of both figures could be clearly seen in perspective without any wandering of the fixation[1]. In the second set of experiments two *different* figures (*i.e.* one drawing of the prism and one of the table) were placed alongside one another in the same way as the two identical ones in the first set, while in the third set the images of two different figures were *superposed* with the help of a stereoscope. In all these experiments the subject was provided with two two-way switches as described above, so that the fluctuations of each figure might be recorded independently, and such additional registering apparatus was provided as was necessary. The process of observing and recording the changes of perspective in the two figures simultaneously was found in the case of all the subjects to be considerably easier than was anticipated, and no serious difficulty was experienced in obtaining reliable records after a little preliminary practice.

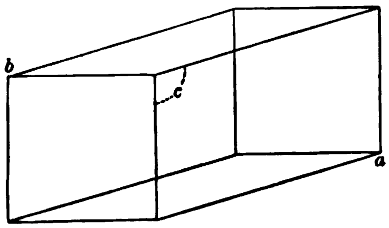

Fig. 1.

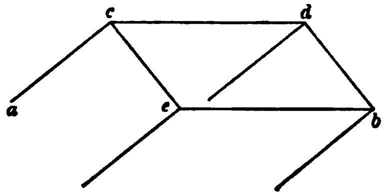

Fig. 2.

The most interesting features of these experiments are shown in Tables I, II and III. Table I contains the results obtained with two identical figures. In this Table are indicated the totals of the individual appearances of each perspective occurring during a single period of observation (in this case one minute). As it was not found possible or desirable to keep the total periods of observation always exactly of the prescribed length (though they seldom varied from it by more than two or three seconds), it has been found convenient to reduce the total of the individual periods during which a particular perspective was seen to a percentage of the total period of observation. Thus, in the columns relating to the prism, the numbers under R indicate the percentage of the total period during which the figure was seen with its right end forward, *i.e.* nearer the observer, those under L indicate the same as regards the time during which the figure was seen with the left end forward, and those under U the same as regards the periods of uncertainty. In the columns relating to the figure of the table, T indicates that the top or upper side of the table was visible, *i.e.* turned towards the observer, B that the bottom or under side was visible and U has

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[1] The drawings were made on slips of cardboard 4·5 cm. long and 2·5 cm. broad, the figure occupying almost the whole of each slip. The eye of the subject was about 40 cm. distant from the table on which the slips were placed. An interval of ·25 cm. (in which was situated the fixation mark) separated the slips from one another.

TABLE I.

Subject	Prism							Table						
	Right figure			Left figure			°/o of opposite phase	Right figure			Left figure			°/o of opposite phase
	R	U	L	R	U	L		T	U	B	T	U	B	
C. R.	47	0	53	28	0	72	32	75	0	25	75	0	25	2
	21	0	79	32	0	68	13	62	0	38	62	0	38	5
J. C. F.	56	0	44	70	0	30	33	57	0	43	58	0	42	2
	72	0	28	53	0	47	43	32	0	68	32	0	68	0
F. A.	72	0	28	67	0	33	11	62	0	38	62	0	38	0
	61	0	39	75	0	25	35	57	0	43	58	0	42	3
C. S.	26	0	74	31	0	69	16	71	0	29	71	0	29	0
	17	0	83	24	0	76	12	54	0	46	51	0	49	10
N. C.	27	20	53	75	20	5	69	42	0	58	31	0	69	28
	12	0	88	100	0	0	88	63	0	37	57	0	43	29

Two juxtaposed drawings of the same figure seen simultaneously. The numerical records refer in each case to the percentage of the total period of observation during which the figure was seen in any particular perspective. The last column to the right in the records for prism and table indicates the percentage of this total period during which the simultaneously observed figures appeared in opposite phase. Two experiments are recorded for each pair of figures with each subject.

For prism: R = 'right end forward.' L = 'left end forward.' U = 'uncertain.'

For table: T = 'top visible.' B = 'bottom visible.' U = 'uncertain.'

the same meaning as before. In this experiment, however, it is more especially the difference between the two figures seen simultaneously that interests us and I have therefore, in a separate column, added the percentage of the total period of observation during which the figures appeared in opposite phase.

An inspection of Table I reveals the following facts of interest for our present purpose:—

1. In all five subjects there are periods in which the figures are in opposite phase.

2. In all cases except two (the observations made by subject N. C. on the prism figure) the total period of opposite phase is less than 50°/₀ of the total period of observation.

3. The two prisms have in every case longer periods of opposite phase than the two tables[1].

4. With both prisms and tables subject N. C. has longer periods of opposite phase than the other subjects.

Introspection showed that the perspective of the figures was just as clear and as easily recognisable during their periods of opposite phase as during their periods of similar phase.

Taken as a whole, the results show clearly that two similar juxtaposed figures seen simultaneously tend to fluctuate together, but that this tendency is insufficient to prevent some periods during which the figures are distinctly seen in opposite phase.

In Tables II and III are shown the most reliable results obtained with two different figures juxtaposed and superposed respectively; these figures being the same prism and table as were used in the preceding experiment. In these tables I have indicated, besides the usual percentages for each figure, the percentage of the total period during which either phase of one figure coincided with either phase of the other. Thus under R (in the last two vertical columns) is shown the percentage during which the prism was seen with the right end forward, which in turn is subdivided into two parts according as it coincided with 'top visible' (T) or 'bottom visible' (B) in the table. It is evident that there exists on the whole a remarkable degree of independence in the behaviour of the two figures, though the results obtained are irregular. These experiments confirm the previous series in showing that two figures seen simultaneously may fluctuate independently of one another, but beyond this the results are chiefly negative. They show no strong tendency for either aspect of one figure to correlate highly with either aspect of the other, nor do they bring to light any marked uniformity of behaviour on the part of any of the subjects. This irregularity of the results shows that the essential factors governing the changes of perspective have been left uncontrolled in these observations and can therefore only be isolated under different experimental conditions. So far as they go, however, these experiments corroborate those recorded in Table I.

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[1] For the probable cause of this see footnote, p. 391.

TABLE II.

Subject	*Juxtaposed* Prism right, Table left							*Juxtaposed* Table right, Prism left						
	Prism		Table					Prism		Table				
	R	L	T	B		R	L	R	L	T	B		R	L
C. R.	0	100	100	0	T	0	100	3	97	37	63	T	3	34
					B	0	0					B	0	63
J. C. F.	92	8	2	98	T	2	0	91	9	0	100	T	0	0
					B	90	8					B	91	9
F. A.	54	46	63	37	T	27	36	47	53	55	45	T	25	30
					B	27	10					B	22	23
C. S.	41	59	52	48	T	25	27	38	62	64	36	T	36	28
					B	16	32					B	2	34
N. C.	3	97	14	86	T	0	14	100	0	20	80	T	20	0
					B	3	83					B	80	0

Two juxtaposed drawings of different figures seen simultaneously. Numerical records refer, as before, to percentages of the total period of observation. To the right of the percentages for the two figures are indicated the percentages during which the different phases of the two figures coincided, the columns referring to the phases of the prism, the (horizontal) rows to the phases of the table. Thus the results obtained with subject C. R. with the prism on the right and the table on the left indicate that the phase 'left end forward' of the prism coincided with the phase 'top visible' of the table during 100 $^{\circ}/_{0}$ of the total period of observation, and that consequently the other combinations 'right end forward' and 'top visible,' 'left end forward' and 'bottom visible' and 'right end forward' and 'bottom visible' did not occur at all.

For prism: R = 'right end forward,' L = 'left end forward.'

For table: T = 'top visible,' B = 'bottom visible.'

In spite of this lack of uniformity in the results, our experiments on two figures seen together afford on the whole some very conclusive evidence as regards the problems on which it was hoped that they would have some bearing. Thus the fact that two figures seen simultaneously exhibit a considerable degree of independence in their fluctuations makes it difficult or impossible to suppose that these fluctuations can depend to any great extent upon some one physiological factor such as the Traube-Hering waves, which would affect the whole of the central nervous system almost at the same time.

TABLE III.

Subject	*Superposed* Prism right, Table left							*Superposed* Table right, Prism left						
	Prism		Table					Prism		Table				
	R	L	T	B		R	L	R	L	T	B		R	L
C. R.	27	73	64	36	T	16	48	19	81	63	37	T	10	53
					B	11	25					B	9	28
J. C. F.	84	16	5	95	T	5	0	87	13	5	95	T	5	0
					B	79	16					B	82	13
F. A.	51	49	63	37	T	32	31	61	39	51	49	T	37	14
					B	19	18					B	24	25
C. S.	48	52	47	53	T	38	9	58	42	57	43	T	33	24
					B	10	48					B	25	18
N. C.	66	34	50	50	T	26	24	28	72	64	36	T	21	43
					B	40	10					B	7	29

Two superposed drawings of different figures; same arrangement as Table II.

Similarly, the results obtained in these experiments are difficult to reconcile with the view that the sole conditions governing the reversals are to be found in fixation and eye movements. According to this well-known theory, that reversible part of a figure which is fixated will invariably appear nearer than that which is seen in indirect vision; and, in the case of eye movements, that point from which the movement starts is seen nearer than that to which the movement proceeds. In the present observations no point of either figure was directly fixated; in view, however, of the recently demonstrated and now generally admitted fact that fixation of any point is never quite accurate for more than a very short space of time, it would seem legitimate to expect on this theory that, in the absence of direct fixation of any part of the figure, that part which is nearest the fixation point would behave as if it were itself fixated. Thus, in the case of the two juxtaposed tables, the part of the right-hand figure which is nearest the fixation point is the end of the leg *a* (Fig. 2); we should therefore expect, according to the above view, that this figure would be seen during the greater part of the time with *a* nearest the spectator, *i.e.* in the phase 'bottom visible.' As regards the left-hand figure, the point *b* is nearest the fixation point, and this figure should therefore appear in the phase 'top visible.' The two figures seen simultaneously under the conditions of our experiment should then have appeared during the greater part of the time in opposite phases, whereas in reality the periods of similar phase greatly predominated. Similarly, in the two juxtaposed prisms, the points *a* and *b* (Fig. 1) were nearest the fixation point in the left- and right-hand figures respectively, and the two figures should likewise have been seen in opposite phases during the greater part of the time, whereas a glance at Table I shows that, although the periods of opposite phase are considerably longer than in the case of the two tables, the total length of all the periods of opposite phase exceeds 50% of the total observation time with only one subject out of five. Again, in the observations with different juxtaposed figures, we should, on the same principles, expect certain combinations to predominate and others to occur but very seldom if at all. Thus, with the prism on the right hand and the table on the left, the table should appear with its 'top visible' and the prism with its left side forward, while the combination 'bottom visible' with right side forward should be very difficult or impossible to bring about, because it would involve fixating the more distant parts of both figures. Nevertheless, the Table indicates that this combination did actually occur with very considerable frequency.

Thus, the experiments made with two figures seen simultaneously seem to give as little support to the view that the changes of perspective can be entirely accounted for in terms of eye movement (or at least to the form in which this view is usually met), as to the other theory which would explain them as due to general circulatory conditions such as the Traube-Hering waves.

III.

In the hope of obtaining more positive results than those described in the preceding section, a fresh series of observations was started under new experimental conditions. These observations were all made with a single figure only, this figure being the well-known truncated pyramid of Mach. The new conditions were of two kinds:—

1. Whereas in the experiments recorded in Section II the attitude of the subject toward the figures had not been specially determined, apart from the general instructions that he was not consciously

to favour any one perspective and that he was, as far as possible, to regard the figures in the same way throughout the observations, the subject was now definitely instructed to adopt one of three attitudes towards the figure, the instruction running: 'Will to see the figure as a chamber' or 'Will to see the figure as a boss' or 'Remain indifferent as to the perspective,' as the case might be.

2. It was thought that some help might be afforded in the attempt to isolate the principal factors upon which the reversals of perspective depend, if some means could be found of gradually making the reversals less easy, so that finally the figure might appear in one perspective only. For this purpose a series of drawings was prepared[1], in which, though starting with the simple Mach truncated pyramid, each subsequent drawing contained an increasing number of details calculated to make the main outlines of the figure (which remained the same throughout) appear in the 'chamber' rather than in the 'boss' perspective. The series consisted of eight such drawings (Figs. 3—10), which are reproduced on the two pages following[2]. The figures were presented to the subjects in the order there given, *i.e.* in order of increasing complication, and with each figure records were made with each of the three attitudes 'indifferent,' 'willing chamber' and 'willing boss' in this order. The experiments were made upon the same five subjects as before, and with every subject one figure only was observed at each sitting, which thus consisted of three periods of one minute each. Only one record was taken for each attitude but only such records were accepted as were considered thoroughly satisfactory both by subject and experimenter, the experiment being repeated whenever necessary, until such a record was obtained.

The principal results of these experiments are shown in Table IV, where, as before, the numbers indicate the percentage of the total time of observation during which a particular perspective was seen. The letters C, U and B over the columns for each figure represent the periods during which the figure was seen as 'chamber,' of uncertain perspective, or as 'boss' respectively. No results are recorded under U for the first two subjects in the Table, as the desirability of recording periods of uncertainty had not become fully apparent until the experiments had been completed with these two subjects.

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[1] I am much indebted to Miss Maud Klein for her kindness in designing and executing the drawings used in these experiments.

[2] Figs. 8, 9 and 10 differ in a few minor respects from the drawings actually used in the experiments. All essential features however (*e.g.* number and position of the persons amount and nature of detail on ceiling, walls and floor) remain unchanged.

An inspection of the Table shows that on the whole the complications in the drawings possess surprisingly little power to produce the favoured perspective and to prevent reversal. With the 'indifferent' attitude, Fig. 8 produces 'chamber' the whole time with only two of the five subjects; in the 'willing boss' observations it produces almost complete predominance of 'chamber' with only one subject. In the 'willing chamber' experiments, however, we find complete or almost complete predominance of 'chamber' in the last four, three and two figures respectively; in subjects N. C., C. S. and J. C. F. Thus, when will and the details added to the figure work in the same direction, they are more effectual than when either is alone. In the record of F. A. there is shown a strong and increasing tendency to avoid seeing the figure as

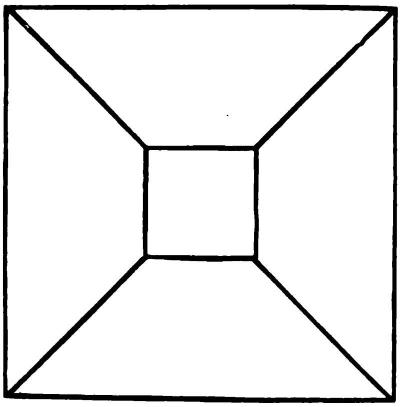

FIG. 3.

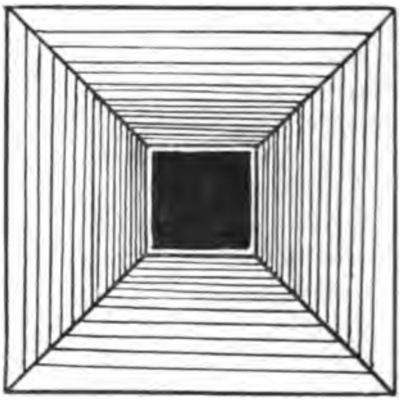

FIG. 4.

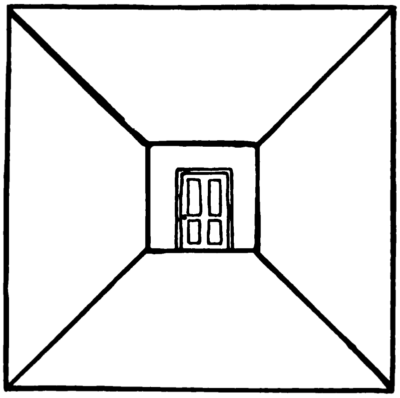

FIG. 5.

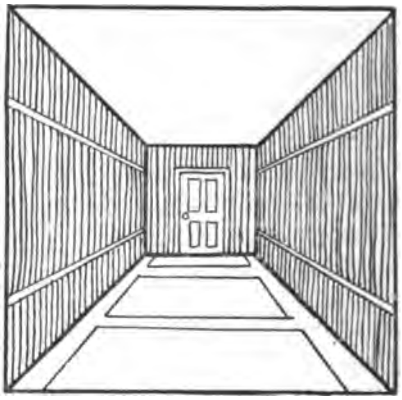

FIG. 6.

'chamber,' which neither will nor increasing complication of the drawing is able to check. In general there appears no constant tendency for the percentage of 'chamber' to increase from figure to figure. No doubt a nearer approach to such a constant tendency would have been revealed, had the average of several observations been taken instead of one observation only for each attitude. Further observations were not made however, since introspection showed that what appeared to be the more fundamental conditions for reversal were not really controlled by the present experiment and at the same time indicated a more hopeful way of investigating these conditions.

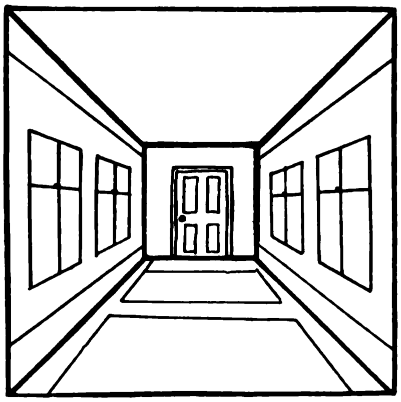
FIG. 7.

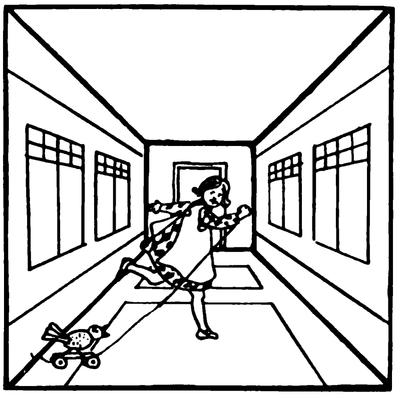
FIG. 8.

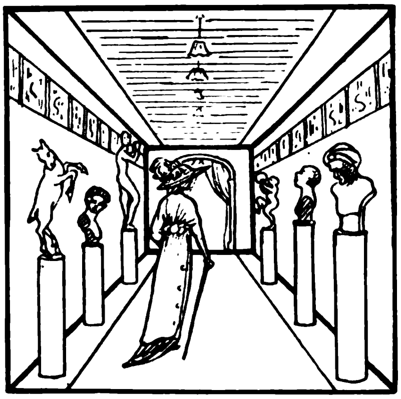
FIG. 9.

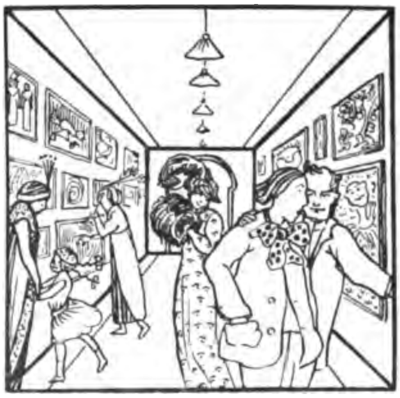
FIG. 10.

It will be noticed that as regards effectiveness of willing, the subjects fall into two fairly well-marked classes—one being represented by N. C. and the other by the remaining four subjects. These individual

TABLE IV.

Observations with the series of drawings, Figs. 3—10. C='Chamber,' U='Uncertain,' B='Boss.'

Subject		Fig. 3			Fig. 4			Fig. 5			Fig. 6			Fig. 7			Fig. 8			Fig. 9			Fig. 10		
		C	U	B	C	U	B	C	U	B	C	U	B	C	U	B	C	U	B	C	U	B	C	U	B
C. R.	Willing C	77		23	65		35	73		27	83		17	74		26	67		33	82		18	84		16
	Indifferent	71		29	57		43	74		26	76		24	71		29	72		28	64		36	70		30
	Willing B	52		48	45		55	68		32	81		19	60		40	68		32	67		33	66		34
J. C. F.	Willing C	80		20	77		23	56		44	90		10	86		14	84		16	95		5	98		2
	Indifferent	79		21	84		16	69		31	85		15	89		11	83		17	98		2	100		0
	Willing B	66		34	54		46	63		37	63		37	73		27	83		17	86		14	95		5
F. A.	Willing C	60	0	40	63	0	37	41	0	59	17	31	52	11	61	28	14	76	10	42	25	33	14	57	29
	Indifferent	50	0	50	52	0	48	40	0	60	22	30	48	6	50	44	0	80	20	39	18	43	0	84	16
	Willing B	42	0	58	25	0	75	26	9	65	7	16	77	2	48	50	0	85	15	9	19	72	10	45	45
C. S.	Willing C	54	4	42	82	6	12	48	44	8	73	20	7	86	14	0	98	2	0	98	2	0	100	0	0
	Indifferent	45	6	49	41	37	22	50	26	24	69	13	18	49	51	0	67	33	0	82	18	0	100	0	0
	Willing B	49	7	44	43	0	57	40	31	29	46	27	27	27	50	23	64	36	0	22	73	5	67	30	3
N. C.	Willing C	87	10	3	88	3	9	84	16	0	93	7	0	100	0	0	100	0	0	100	0	0	100	0	0
	Indifferent	33	27	40	25	75	0	27	65	8	13	24	63	82	18	0	47	8	45	34	66	0	67	33	0
	Willing B	5	5	90	29	7	64	17	0	83	0	0	100	74	6	20	0	13	87	0	0	100	0	8	92

differences in the power to control the reversals of perspective are among the most interesting facts brought to light by the present investigation. They will be met with again in the later experiments, but in order not unduly to complicate the problem with which we are here primarily concerned, I shall defer a discussion as to the real nature of these differences until after the presentation of some further data on the subject, which I hope to bring forward in a subsequent paper.

Although it does not appear in Table III, it may be mentioned that will had little influence on the average duration of all the periods composing a single observation, the lengthening of the periods of the desired perspective being compensated by a corresponding shortening of the undesired perspective.

The most useful and interesting results of this series of experiments are probably to be found in the introspections. These explain to some extent the cause of what is at first sight the rather surprising inability of the details of the figures to prevent reversals of perspective, and at the same time bring us a step nearer to the end we have in view—the discovery of the true psychological conditions upon which the reversals depend. The most fundamental fact brought out by the introspections of all the subjects is that, in nearly every case where the details of the more complicated figures failed to prevent reversals of perspective, these details were less *attended to* than were the main outlines of the figure. As regards the observations with 'indifferent' attitude, there are at least two conditions of the experiment which may be supposed to have played a part in producing this comparative neglect of the details. There is first the general instruction to the subjects to observe and record any reversals of perspective that occur, which in the absence of any instruction to the contrary, would naturally tend to make the subject concentrate his attention chiefly on the main outlines, since it was here alone that reversal could be observed. There is, in addition, the fact that with all the subjects the earlier observations were made upon the simpler figures, which contained only the main outlines or but few complicating details, and the attitude necessarily adopted in the case of the early figures seems to have perseverated to a considerable extent in the observations made with the later figures. This is expressly mentioned by two subjects and seems to have occurred also in the other cases.

All the subjects seem to have spontaneously adopted distinct attitudes in the case of the 'willing boss' and 'willing chamber' experiments respectively. In the case of 'willing chamber' there seems to have been generally a more or less conscious endeavour to concentrate attention on the details of the figures and to allow them to enter clearly into consciousness. Comparative neglect of details on the other hand is reported in nearly every case where 'willing boss' was conspicuously successful. In the few remaining cases those details which still remain prominent seem to have been incorporated into the 'boss' perspective by acquiring some definite meaning in connexion with the boss, *e.g.* by becoming lines or marks upon the sides of the boss. An extreme instance of this is when the details of Fig. 10 were seen as the ornamentation upon a 'Japanese paper basket,' the bottom of which was towards the observer. In a very few instances a detail might remain clear, but fail to be incorporated into the predominant boss perspective. Thus, the child in Fig. 8 was seen by one subject to have no connexion with the boss, but to be superposed upon it in some way as an irrelevant detail, much as an insect or a piece of dirt lying on the paper. In extreme cases, however, inability to neglect the details, in spite of a high degree of attention to the main outlines, would bring about a splitting up of the figure into two incongruous perspectives. Thus, in Fig. 7, 'chamber' would suddenly change into 'boss' while the windows retained their original ('chamber') perspective. One subject describes an appearance of this sort as like looking through a transparent boss into a corridor. While willing 'chamber,' on the other hand, the subjects seemed to have allowed the attention to move freely over the details of the figure and to have welcomed them as aids to the maintenance of the desired perspective. Several subjects report that failure to preserve the 'chamber' perspective during the whole period, even with the later figures, was due to sudden irruptions into undue prominence of the main outlines and particularly of the inner square, resulting in a momentary neglect of the details.

Thus the introspections seem to indicate that the direction of the attention plays a very important part in the reversals of perspective. In the figures used in the present experiments, clearness in consciousness of the main outlines seems to have been essential to the appearance of 'boss,' while prominence of the details was a great aid to the maintenance of the 'chamber' perspective. The regularity with which these facts came out in the introspections of all the subjects seemed to indicate that they were of fundamental importance, but in themselves they are unable to explain the changes of perspective in the usual kind of ambiguous figures, since here there is no complicating detail. However, the manner in which the direction of attention operates as a decisive factor in these cases also is brought out clearly in the experiments described in the next section.

IV.

As it was by now probable that some at least of the most essential conditions governing the reversal of the perspective were to be found in the attitude of the subject towards the figure, and in particular, in the relative amount of attention directed to different parts of the figure, a fresh series of experiments was undertaken, in which the subject was instructed to adopt a different attitude towards the figure during successive observations, while the figure itself and all the external conditions of the experiment remained the same throughout, and were, unlike those of the previous observations, of the greatest possible simplicity. For this purpose use was made of the prism (Fig. 1) already employed in the observations described in Section I, experience having shown that on the whole both the perspective itself and the changes from one perspective to another were clearer in this figure than in most others. A list of ten attitudes was drawn up, these attitudes being chosen from among those which had at one time or another been spontaneously adopted by one or more of the subjects in the earlier experiments, and which had been described in their introspections. Each attitude was adopted once at every sitting, which therefore consisted of ten observations of one minute each. In order to avoid as far as possible any effects of the order in which the attitudes were taken and of the perseveration of one attitude upon the succeeding one, the experiments were so arranged that each attitude occupied every possible place as regards order and was preceded and succeeded by every other attitude in the course of ten sittings[1]. The actual influence of order and perseveration appears however to have been very small as soon as the subjects had become thoroughly used to the attitudes and to the general conditions of the experiment. A small fixation mark was provided in the middle of the figure, and the subjects were instructed to do their best to fixate this mark steadily throughout the experiments, but not to let the effort to fixate interfere seriously with the adoption of the required attitude.

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[1] As this arrangement has not, so far as I am aware, been described before, and as it is probable that other cases may frequently arise, in which, when repeating a number of performances on different days, it is desirable to neutralise any effects both of the order of presentation and of the perseveration of the mental attitudes occasioned by the various performances, it is perhaps worth while to indicate the method here. For the sake of simplicity let us suppose that we are dealing with four performances only, instead of with ten, as in the present case. Let us designate these four performances 1, 2, 3 and 4 respectively, and take them in this order on the first day. If now for the sake of calculation we insert a 0 before the 1, the rule for the order in which the performances are to be taken on subsequent days is as follows:—

The numerical results obtained from these experiments with the three subjects on whom they were carried out are shown in Table V, in which are given both the percentage of the total time of observation during which either perspective was seen (as in the earlier Tables), and the average duration of the periods of either perspective. As, however, the value of the experiments depends even more upon the introspections of the subjects than upon the numerical results, we will proceed to consider the nature and effect of each attitude in turn, with particular reference to the introspections obtained from each of the three subjects.

It will materially facilitate recognition of the general conclusion to be drawn from these observations, if the reader is informed beforehand of its nature (which was of course only clear to the experimenter at the end of the whole series of experiments), and is thus able to consider the individual results obtained from each attitude in the light of the general result obtained from all. The present experiments make abundantly clear what was already indicated in the previous series of observations—that the nature of the perspective in which an ambiguous figure is seen at any moment is (at least under the present experimental conditions and for the present observers) mainly determined by the

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For the second day begin with 2 and take in succession each second performance in the order in which they were taken on the first day.

For the third day begin with 3 and take in succession each third performance, and so on till each performance has been taken first on one occasion.

The scheme for our four performances will thus be as follows:—

Day	Order			
1	1	2	3	4
2	2	4	1	3
3	3	1	4	2
4	4	3	2	1

This arrangement of course necessitates a number of sittings equal to the number of performances or some multiple of this. Unfortunately it does not admit of application to an *uneven* number of performances without considerable modification entailing the non-utilisation of some of the data.

TABLE V.

Attitudes	I.			II.			III.			IV.			V.		
	L	U	R	L	U	R	L	U	R	L	U	R	L	U	R
Subject: C. S.															
°/o	45	2	53	38	2	60	33	33	34	42	0	58	40	17	43
m. v.	*3·6*	*3·0*	*5·0*	*3·9*	*1·8*	*4·9*	*8·2*	*8·1*	*6·9*	*5·3*	*0*	*5·3*	*9·5*	*9·8*	*5·3*
Av. duration	4·6	1·2	5·5	4·2	1·2	6·6	2·6	2·5	3·2	5·1	0	7·0	4·0	2·0	3·7
m. v.	*1·4*	*0·3*	*2·1*	*1·2*	*0·3*	*1·8*	*0·8*	*0·7*	*1·3*	*1·0*	*0*	*1·2*	*1·4*	*0·2*	*1·2*
Subject: A. W.															
°/o	25	44	31	28	45	27	11	77	12	56	1	43	7	83	10
m. v.	*17·7*	*40·0*	*22·7*	*15·7*	*29·5*	*14·3*	*8·5*	*13·2*	*6·0*	*7·0*	*1·8*	*6·8*	*1·7*	*7·8*	*6·2*
Av. duration	2·3	15·9	2·5	2·2	14·0	1·8	1·5	16·5	1·5	6·3	4·0	5·0	1·3	10·3	1·7
m. v.	*0·8*	*13·4*	*1·2*	*1·3*	*17·2*	*0·8*	*0·5*	*14·5*	*0·4*	*1·4*	*0*	*0·8*	*0·3*	*3·8*	*0·8*
Subject: N. C.															
°/o	4	32	64	3	71	26	0	100	0	0	7	93	0	100	0
m. v.	*5·6*	*20·8*	*17·8*	*4·6*	*32·5*	*28·7*	*0*	*0*	*0*	*0*	*12·1*	*11·6*	*0*	*0*	*0*
Av. duration	3·1	8·6	18·1	1·4	27·9	8·8	0	60	0	0	10·1	45·2	0	60	0
m. v.	*1·4*	*6·3*	*14·3*	*0·2*	*24·6*	*8·3*	*0*	*0*	*0*	*0*	*8·3*	*20·6*	*0*	*0*	*0*

Attitudes	VI.			VII.			VIII.			IX.			X.		
	L	U	R	L	U	R	L	U	R	L	U	R	L	U	R
Subject: C. S.															
°/o	45	7	48	32	4	64	37	11	52	41	6	53	50	4	46
m. v.	*7·6*	*5·6*	*8·2*	*6·6*	*4·9*	*5·9*	*8·5*	*7·3*	*6·8*	*6·6*	*6·3*	*6·1*	*5·2*	*3·9*	*7·1*
Av. duration	4·0	1·2	3·5	2·5	3·3	4·8	3·6	1·6	3·6	3·0	1·5	3·8	5·1	1·6	3·7
m. v	*1·7*	*0·3*	*0·5*	*0·2*	*0·4*	*1·2*	*0·3*	*0·4*	*0·7*	*0·8*	*0·3*	*1·0*	*1·3*	*0·4*	*1·1*
Subject: A. W.															
°/o	82	4	14	44	7	49	86	4	10	53	12	35	83	3	14
m. v.	*6·2*	*4·2*	*3·3*	*13·0*	*8·5*	*16·2*	*7·2*	*2·5*	*9·0*	*3·0*	*10·7*	*10·0*	*3·0*	*3·5*	*3·5*
Av. duration	7·6	1·3	1·4	3·5	2·8	4·1	15·9	2·2	2·1	2·1	1·4	1·5	8·3	1·9	1·2
m. v.	*1·6*	*0·3*	*0·3*	*1·0*	*0·7*	*1·0*	*8·7*	*0·6*	*0·6*	*0·7*	*0·3*	*0·4*	*1·9*	*0·1*	*0·1*
ubject: N. C.															
°/o	100	0	0	—	—	—	95	5	0	100	0	0	100	0	0
m. v.	*0*	*0*	*0*	—	—	—	*5·8*	*6·0*	*0*	*0*	*0*	*0*	*0*	*0*	*0*
Av. duration	60	0	0	—	—	—	44·7	3·7	0	60	0	0	60	0	0
m. v.	*0*	*0*	*0*	—	—	—	*18·3*	*2·6*	*0*	*0*	*0*	*0*	*0*	*0*	*0*

Observations on the prism figure with different attitudes. The Table indicates (i) the percentage of the otal period of observation during which the figure was seen in any particular perspective, (ii) the average uration of the individual periods of each perspective. The *percentage results* give the average for the hole period of ten sittings. In cases where any perspective was not observed on every sitting, the results iven for the *average duration* refer only to the total number of sittings in which this perspective was observed nd not to the whole period of 10 sittings. For description of attitudes see text.

L = 'left end forward,' U = 'uncertain,' R = 'right end forward.'

direction of the attention; and that the direction of the attention determines the perspective according to the following rule:—

As regards all reversible parts of the figure, that part of the figure which receives the more attention is seen in front of that part which receives the less attention[1].

Having thus given in anticipation the general conclusion to be drawn from the whole series of experiments, we will proceed, as briefly as possible, to describe each attitude, to examine its effects as manifested through the numerical results and through the introspections, and to show how these effects of the individual attitudes support the above general conclusion.

Attitude I may be described as that of passive contemplation. The instructions to the subject were as follows: "Contemplate the figure as passively as is compatible with observing and recording the changes of perspective that occur. As far as possible keep out everything from consciousness except the figure you are contemplating, and refrain from willing or wishing to maintain or to change any perspective." The attitude thus reduced all conscious effort to a minimum. Corresponding to the reduced attention there is frequently reported a reduction in the vividness of the perspective (hence frequent large amounts of U with two of the subjects); but as no attempt was made to control the direction of such attention as there was, the conditions were not favourable for bringing out any correspondence between the direction of the attention and the nature of the perspective. Two of the subjects seem to have found the maintenance of this attitude a pleasant and easy task, disturbed only, in the case of N. C., by occasional doubts as to whether or not there was any perspective at all (and hence considerable variations in the amounts of U were found in her records). To A. W. however the attitude presented some difficulty, owing to the fact that (in his own words) "the endeavour to record any changes of perspective frequently occasioned a turning of the attention to one side or the other to see if that side were forward or back," and, as the side which received this increased amount of attention invariably appeared forward, this procedure seemed to contravene the instruction to be passive and indifferent. In the earlier sittings A. W. also reported the interesting phenomenon of both ends of the figure being forward simultaneously, but subsequent introspection showed this to consist in an extremely rapid oscillation between the two perspectives, accompanied by equally rapid changes in the direction of attention. These introspections of A. W., together with the loss of clearness of perspective in the case of N. C., are of course in strict agreement with the general conclusion mentioned above. Perhaps the chief use of this attitude, however, does not lie in the results obtained, considered by themselves, but in the fact that it afforded a standard of 'normal' or 'natural' observation for judging the results of the subsequent attitudes, in which the direction of the attention and the other conditions of observation were more precisely determined.

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[1] In view of the many senses in which 'attention' has been understood by different writers, and of the many and varied features which can be introspectively discovered as characteristic of the attentive consciousness, it becomes highly desirable to define more precisely the meaning of the word as it is used here. It is above all intended to signify something predominantly cognitive in nature. Although conative and affective elements are often, perhaps always, present, they are not the most constant or the most characteristic features of what is here called attention, which is perhaps most accurately described as 'clearness in consciousness.' Thus when we say that a particular part of a figure receives the most attention, we mean primarily that that part is more clearly in consciousness than the remainder of the figure. This 'clearness' however must be carefully distinguished from (1) sensory intensity, and (2) sensory distinctness, *i.e.* sharpness of differences as presented in sensation, of both of which it would seem to be very largely independent. The relation of 'clearness' to intensity of mental process as a whole presents a more difficult problem, since such intensity is dependent more on conative and affective than on purely cognitive factors. This problem, however, though of considerable interest in itself, is not of great importance for our present purpose, since it is the cognitive elements which varied most in our observations, the conative and affective elements, and therefore also the total intensity of consciousness, remaining (with few exceptions) comparatively unchanged from one experiment to another.

Attitude II may be described as being one of distributed attention. The instructions were: "Contemplate the figure with a high degree of attention, this attention to embrace continuously the whole of the figure. Refrain from willing or wishing to maintain or to change any perspective." In A. W. these instructions seem to have produced two distinct attitudes—(1) a really successful attempt to embrace the whole figure simultaneously, (2) in default of this, a continuous rushing of the attention over the whole figure, taking in the different parts in rapid succession. (1) seems to have produced long periods during which the figure was seen without perspective; (2) very short periods of alternating perspective. This difference accounts for the high m. v. of the results obtained with A. W. from this attitude[1]. The introspections of N. C. show that during the earlier observations she had some difficulty in keeping the whole of the figure steadily in attention, and that as soon as the distribution of attention became conspicuously uneven, that part of the figure which received the more attention was seen forward. In the later observations the attention is reported as being more successfully distributed over the whole figure, with the result that the figure is seen without perspective. Thus the introspections and numerical results obtained from A. W. and N. C. are in full agreement with the general conclusion. When the attention is equally distributed over the whole figure no perspective is seen, since neither of the reversible parts of the figure receives a greater amount of attention than the other. Less characteristic results were however obtained from C. S. The effect of the different attitudes is indeed throughout less marked with this subject than with the others. In the present case this lack of effect is increased by the fact that in C. S. the tendency to see the figure in some kind of perspective is apparently considerably more marked than it is with the other subjects (this tendency being manifested in smaller amounts of U throughout the experiments). This probably counteracted the natural tendency of the attitude to make the figure appear flat. As, however, this subject also appeared to experience greater difficulty in maintaining a steady distribution of attention over the whole figure, the results are not opposed to our general conclusion.

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[1] The various other cases of unusually high mean variation with this subject and with N. C. are, almost without exception, similarly traceable to noticeably different distributions of the attention having been adopted on different sittings.

Attitude III demanded concentration in place of distribution of attention. The instructions were as follows: "Contemplate the figure with a high degree of attention, this attention being concentrated on the central part of the figure. Refrain from willing or wishing to maintain or to change any perspective." The effect of the attitude is shown unmistakeably in larger amounts of U with all three subjects. Concentration on the centre of the figure produces a corresponding neglect of the sides and inability to see the figure in perspective. In the case of A. W. and C. S. this is perhaps partly due to the ends of the figure being insufficiently in consciousness for the subject to be certain whether they were in perspective or not; it is, however, beyond doubt due also in part to a general flatness of the figure. As before, short appearances of one perspective or the other usually coincide with observed movements of the attention to the corresponding side. With N.C. there seems never to have been any doubt as to the flatness of the figure during the whole time. "The sides were nevertheless sufficiently in consciousness for any perspective to have been noticed, had there been any." Thus the results are again in harmony with our general conclusion; when attention is withdrawn from either of the reversible parts the figure is seen without perspective.

For Attitude IV the instructions were as follows: "Contemplate the figure with a high degree of attention, this attention being concentrated on the end which happens to be forward for the time being. Refrain from willing or wishing to maintain, or to change any perspective." The effect of the attitude is, as might be expected, to increase the length of the periods during which either perspective persists uninterruptedly, since as soon as any perspective appears, attention is so directed as to make the conditions (according to our general rule) most favourable for its persistence. With N. C. this attitude suffices to maintain one perspective (the 'right forward' or naturally favoured one, in the absence of any instructions to the contrary) nearly the whole time. A. W. reports that he could have done the same, if only he could have prevented the attention from wandering off to the other side. C. S. also frequently observes that the changes of perspective coincide with a noticeable relaxation of attention to the side that had been forward. Both A. W. and C. S. are powerless to prevent these relaxations or wanderings of attention, though of course their occurrence may be considerably delayed by an act of will, as in the present case. The results obtained from this attitude are then again in strict conformity with our general conclusion.

Attitude V. Up till now the subjects had been instructed to preserve a neutral attitude towards the figure. This was now abandoned in favour of a distinct effort of will to see the figure in some definite manner. The instructions for Attitude V were as follows: "Will to see the figure without perspective, but take care that the whole figure remains in consciousness." With all three subjects the willing seems to have been positive, and not, as the instructions might suggest, negative; *i.e.* they willed to see the figure *in* some definite way or *with* some definite appearance, not *without* the appearance of perspectivity. With A. W. and N. C. the attitude seems to have become very like that of the distributed attention in Attitude II; their efforts were directed chiefly to distributing the attention evenly over the whole figure; when this was successfully accomplished, the absence of perspective followed automatically without any further effort of will. C. S. seems to have relied chiefly upon seeing the figure as a geometrical design, a method which was also from time to time adopted by the other subjects. In so doing, those parts seem as a rule to have been most clearly in consciousness which are least connected with either of the dominant perspectives. It is interesting to note that this subject records a smaller amount of U when (as in this case) he is actually willing to see the figure without perspective, than in Attitude III, when he is concentrating attention on the centre. The effort to keep the whole of the figure well in consciousness (demanded by the present instructions) makes it necessary to give a good deal of attention to those parts of the figure most closely associated with the perspective, and it is probable that a very slight increase of this attention suffices to bring in the perspective, whereas the danger of this happening is considerably reduced when the whole of the attention may be concentrated on the centre of the figure. This indicates that the direction of the attention is of much greater importance than any mere effort of will in determining the presence or absence of perspective. This is fully in harmony with the results obtained from the subsequent attitudes and with the introspections of the subjects.

For Attitude VI the instructions were: "Will 'left forward' (by this was meant in this and the subsequent attitudes—'Will the figure to appear in such a perspective that the left end is forward') by pulling the left end towards you." In the introspections obtained from this attitude all three subjects report that both willing and pulling are in themselves of no assistance in producing the desired perspective, which can be obtained just as well by the mere concentration of attention upon the left square. The subjects are, however, successful in producing this perspective (even with C. S. the amount of L is only surpassed in one other attitude) just because the attitude permits and demands the direction of attention principally upon the left square. As before, it is reported that appearances of 'right forward' frequently coincide with observed fluctuations of the attention in the same direction.

Attitude VII was as follows: "Will 'left forward' (in the abovementioned sense) by pushing the right end away from you." All three subjects find this attitude one of great difficulty and have but poor success in producing the desired perspective. N. C. indeed says that she finds the attitude itself impossible to attain, and failed to produce what she could regard as a satisfactory record after the first two attempts. Pushing the right end back involves paying attention to that end, and as soon as the necessary amount of attention was given to the right end, it came forward with such vividness as to make it appear hopeless and impossible to will to see the figure in any other perspective. A. W. also described the attitude as being very difficult, and only achieves some measure of success by the help of an artifice;—in contemplating the right square he directs his attention particularly to the right-hand part of the diagonal line joining the upper right-hand corners of the two squares contained in the figure, and sees *this* in front of the right-hand square itself. This of course produces the desired perspective of the whole figure and is in accordance with our general rule; to judge from the introspections however, this procedure seems to have alternated rather that coincided with the pushing back of the right square demanded by the instructions, which, in itself, is probably as inconsistent with the appearance of 'left forward' as in the case of N. C. In agreement with this is the fact that A. W.'s willing, with this attitude, is distinctly less successful than usual. With C. S. this attitude, in spite of the willing, actually produces a smaller amount of 'left forward' than any other. The introspections from this subject show that whenever the left side was forward there was a considerable relaxation of attention to the right square and a corresponding increase of attention to the left, the willing to push back the right square, if continued at all at these moments, being performed only "in a helpless Platonic sort of way."

In Attitude VIII willing was combined with concentration of attention upon quite a small area of the figure. The instructions were: "Will 'left forward'; in so doing concentrate the attention upon the angle indicated," this angle being the one indicated as *c* in Fig. 1. The results from this attitude, while confirming those already obtained, do not give any fresh information of particular interest. The attitude is not difficult and the willing is attended with a considerable amount of success, since attention can be concentrated on a point which it is desired to see forward. The introspections indicate, however, that at the moments when the 'left forward' perspective was clearest, a considerable amount of attention was given to the left square as well as to the angle.

Attitude IX. The instructions for this were: "Will 'left forward' by means of preperception of the desired perspective, when it is not present. When it is present, adopt the 'passive contemplation' of Attitude I." For two of the subjects (A. W. and C. S.) this attitude resolved itself into an alternate strengthening and relaxation of attention and will. Preperception and will, however, were found, here as before, to be of no importance, the perspective apparently being determined solely by the direction and the amount of the attention. All three subjects present interesting individual characteristics in the results obtained from this attitude. A. W. exhibits most clearly the effects of the alternating increase and relaxation of attention in a considerable shortening of the periods during which each perspective lasted. As soon as he had obtained the desired perspective by means of vigorous attention to the left square, he relaxed attention according to the instructions. This was immediately followed by a reversal of the perspective, frequently accompanied by a noticeable increase of attention to the right side. This reversal necessitated a fresh concentration of attention upon the left square, which brought back the 'left forward' perspective, and so on. With C. S. these alternations seem to have been almost equally well marked subjectively, but, owing to the fact that there was considerable inertia in passing from the state of strained to that of relaxed attention, the periods are not appreciably shorter than usual. With N. C. there are few if any reversals of perspective with this attitude. Judging from the introspections, the initial stage of the attitude was much the same as with the other subjects, but the relaxation of attention does not, as with them, seem to have involved a marked increase of the relative amounts of attention given to the right square. The absolute amount of attention was decreased without undergoing any considerable change of direction. Thus the 'left forward' perspective, when once obtained, was in the majority of cases enabled to persist uninterruptedly during the whole time of observation.

The tenth and last attitude was as follows: "Will 'left forward' by means of calling up suitable auxiliary images or ideas." In some preliminary experiments, as well as in several of the experiments recorded in the earlier sections, there had appeared to be some evidence in favour of the view that 'willing' was both easier and more effective when the figure was given some definite meaning, or incorporated into some system of imagery or ideas tending to make it appear in the desired perspective, than when attention was merely concentrated on the figure as actually presented. The present attitude was included in the series with a view to obtaining some further evidence on this point. It would seem, however, that this influence of the method of willing, if it exists at all, can only manifest itself in so far as it affects the direction of the attention. There is perhaps some slight evidence in the case of C. S. to show that it is easier to keep the attention fixed upon the left square, by this indirect, than by the more usual direct method; but our data do not enable us to say whether this is a general rule, or is peculiar to this subject.

The experiments we have described in this section seem to supply on the whole a very consistent body of evidence in favour of the general conclusion we have already enunciated, viz. that, as regards all reversible parts of the figure, that part of the figure which receives the more attention is seen in front of that part which receives the less attention. Before going on to consider how far this rule, which seems so clearly demonstrated in the case of the present observations, admits of application to all cases of reversible perspective, it may be well to draw attention to the interesting individual differences between the three subjects of the experiments recorded in this section. As already indicated, I intend to postpone a consideration of the underlying causes of these individual differences until I am in a position to present some further data on the subject; but a brief summary of the actual facts as revealed in the present experiments may not be out of place here.

The principal feature of these individual differences is concerned with the power of voluntary control over the perspective, or, in other words, the power of producing and maintaining a given distribution of the attention. N. C. possesses this power of voluntary control in a remarkably high degree; with C. S. it is present only to a comparatively small extent, while A. W. occupies an intermediate position in this respect. Throughout the attitudes N. C. exhibits the results of a very stable condition of the attention. Thus the 'distributed attention' in Attitude II produces a much larger amount of U than in the case of A. W. or C. S., since the attention, once evenly distributed over the whole figure, is less liable to fluctuations than with the latter subjects. The same difference is almost equally marked in the case of the 'concentrated attention' in Attitude III. In Attitude IX relaxation of the attention does not lead to its redistribution and the consequent change of perspective, as it does with the other subjects. In Attitude IV concentration of attention on the end which happens to come forward suffices in most cases to keep that end forward during the whole period of observation. The same applies to all the 'willing' attitudes. In Attitude VII however, in which willing 'left forward' had to be combined with attention to the right, this remarkable stability of attention seems to have been a positive disadvantage. As soon as the necessary attention was given to the right square to 'push it back,' it came forward with such persistency as to make it appear impossible to see the figure in any other perspective without removing the attention from this square, and thus abandoning the 'pushing back.'

With A. W. there is a tendency for the attention to fluctuate very rapidly in the absence of any effort to keep it constant (cf. the very rapid fluctuations resulting in the appearance of 'both ends being forward simultaneously' and the short periods resulting from the alternate strengthening and relaxation of attention in Attitude IX). Nevertheless this subject possesses very considerable powers of voluntary control over the direction of the attention, and the general results obtained from the different attitudes are (except in the case of IX) in the same direction as those of N. C., but are less marked; *e.g.* IV produces longer periods but no complete predominance of one perspective, VII is difficult and the willing is not successful, but the attitude is not pronounced impossible.

C. S. possesses the least voluntary control over the direction of the attention, and is as a rule, even under favourable conditions, unable to maintain either perspective uninterruptedly for any length of time. He shows, however, a more marked tendency than either of the other two subjects to see the figure in some kind of perspective, as is shown by the very small amount of U to be found throughout his records. As a consequence of these characteristics the effect of the different attitudes is generally much less marked than it is with the other subjects, but is nevertheless usually in the same direction; *e.g.* there is an increased amount of U in III; there is a distinct lengthening of the periods in IV; and 'willing' meets with least success in VII, though the attitude is not so difficult as it is to A. W. (and therefore still less difficult than it is to N. C.).

V.

Although the results detailed in the last section seems thus to indicate throughout that the direction of the attention is the principal factor determining the nature and the reversals of perspective, it may perhaps be objected that, in obtaining these results, we have insufficiently guarded against the intrusion of another factor, namely the influence of fixation. At the conclusion of the last set of experiments, however, both the experimenter and the observers were strongly of opinion that variations in the nature of eye movement and of fixation were utterly inadequate to account for the often markedly different results obtained with the different attitudes. Eye movements and fixation were frequently mentioned in the introspections of all three subjects, and, except in a very few cases, the eye movements were neither sufficiently extensive nor sufficiently regular for it to appear possible that they could have influenced the results in any considerable manner. These considerations were supported by the results of the experiments with two figures seen simultaneously described in Section II, which, as we saw, are difficult or impossible to reconcile with the view that would regard eye movement as the main principle or explanation of the phenomena of reversible perspective. They are likewise in agreement with the observations of most other recent investigators, who seem to be of opinion that changes of perspective may occur without the corresponding eye movements which should, according to a widely held theory, invariably accompany them. In view, however, of the extreme importance which has been attached by some writers to the influence of eye movement and fixation, it seemed desirable to submit our results to a severer test than was afforded by the instructions regarding fixation in the experiments recorded in the last section and by the introspections on this point incidentally obtained in the course of these experiments. For this purpose a special set of observations was made, with the same figure and under the same general conditions as these experiments, and with the same subjects as had taken part in them. For the small ink fixation point in the middle of the figure there was, however, substituted a small piece of black paper, 2·5 mm. square, the actual fixation point being given by a pinhole in the centre of this. After this had been fixated for a few seconds in a bright light (such as was used throughout our experiments) any wandering from the fixation point could be observed by means of the negative after-image of the black square, which appeared as a very white patch upon the duller white of the paper on which the figure was drawn. By this means the subjects were able, after a little practice, to note and to report both the direction and the extent of their eye movements[1]. It was found that when the subjects fixated the centre of the figure and at the same time attended to one or other of the ends (as was done in the 'attitudes experiments') the after-image seldom, if ever, went into the square to which the attention was directed, and was indeed seen only as a narrow white strip immediately in contact with the black square. The subjects, who by this time possessed a very considerable amount of practice in this kind of observation, were quite positive in their assertions, and the observations were repeated often enough to show that the reports remained constant from one sitting to another. Although the eye movements were thus found to be very small, if not altogether negligible, the nature and reversals of the perspective followed the direction of the attention exactly as before.

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[1] Similar observations had already been made by Becher (*op. cit.*) with much the same results as those obtained in the present case.

TABLE VI.

Attention Fixation	Left Left			Left Centre			Left Right			Right Left			Right Centre			Right Right	
	L	U	R	L	U	R	L	U	R	L	U	R	L	U	R	L	U
Subject: C. S.																	
Attention primary	95	0	5	64	15	21	84	0	16	43	0	57	5	0	95	19	0
Fixation primary	95	0	5	29	21	50	46	0	54	45	0	55	15	0	85	33	0
Subject: N. C.																	
Attention primary	100	0	0	100	0	0	54	46	0	0	[illegible]	20	0	0	100	0	0
Fixation primary	73	0	27	100	0	0	0	100	0	0	100	0	0	0	100	0	
Subject: J. C. F.																	
Attention primary	83	5	12	70	0	30	22	48	30	18	61	21	31	7	62	24	
Fixation primary	76	20	4	55	0	45	9	60	31	53	17	30	33	5	62	31	

Observations on the influence of simultaneous attention and fixation to various parts of the pris attention and fixation being made in turn the primary and secondary task respectively. Numerical resu to percentages of the total period of observation.

L = left and forward, U = uncertain, R = right and forward.

This, while showing that the changes of perspective can be independent of fixation and eye movements, does not of course prove that they actually were so in the 'attitudes experiments,' since the conditions were not the same in these experiments as in the special observations on fixation just reported. The effort to fixate and to observe the movements of the after-image in the latter case was greater than that usually devoted to fixation in the 'attitudes experiments,' where the subject was expressly directed not to allow the effort to fixate to interfere at all with the adoption of the required attitude. In order to obtain some idea of how far there existed a mutual interference between strict fixation and observation of the after-image on the one hand and successful attention to the squares on the other hand, a few further observations were made, in which the subjects were alternately instructed to devote their primary efforts

(1) to the maintenance of the fixation and observation of the after-image, and (2) to the concentration of the attention upon one of the squares, all other conditions meanwhile remaining unchanged.

In these experiments three fixation points were used—the usual one in the centre of the whole figure and one in the centre of either square. With each fixation point, attention was directed in turn to either square. Some typical results are recorded in Table VI. These refer, as usual, only to the perspective (and hence indirectly to the attention given to the squares), since it is only with regard to this that an objective record was obtained. These observations show that very accurate maintenance and recording of the fixation and simultaneous attention to another part of the figure do, to some slight extent, inhibit one another, but that this mutual inhibition is never sufficiently great as to make the results obtained under the two sets of conditions totally incomparable[1]. We thus seem fairly justified in assuming that the results of the 'attitudes experiments' themselves are not due to fixation any more than are the results obtained in the experiments in which fixation was specially controlled; and we saw that, in the latter case, attention to either square was capable of bringing forth the corresponding perspective, under circumstances in which eye movements sufficient to account for the perspectives could be definitely observed not to occur. The most that can be said as regards the influence of fixation in the 'attitudes experiments' is, that, if sufficient attention had been paid to the fixation as to preclude the possibility of any appreciable effect of eye movements (as was the case in the special fixation experiments), then the attention to the squares would have suffered to some extent, and the variation of the results with the different attitudes would have been less marked, though they would still have been in the same direction as were the results which were actually obtained. Of any real explanation of the results in terms of eye movement and fixation, there can, in our opinion, be no question.

VI.

Having thus disposed of the objection that the results apparently due to the direction of the attention are in reality to be attributed to eye movements and fixation, it remained to be seen how far the general rule as regards the influence of the direction of the attention on the perspective, so clearly demonstrated in the case of our prism figure, holds true of other cases of reversible perspective. For this purpose a number of observations were made with several other well-known figures; in these observations a point in the centre of the figure was fixated by the aid of a small square of black paper, as in the experiments described in the last section (though the subjects were no longer instructed to regard fixation as the primary task), and the attention was then concentrated upon various parts of the figure in turn, the resulting perspective being recorded in the usual manner.

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[1] In the absence of an objective record, it is difficult to speak with certainty of the amount of deterioration undergone by the fixation, when efforts were directed primarily to concentration of attention on the squares. If we rely on the introspections, it seems to have been somewhat similar in amount to that undergone by the attention during the primacy of fixation.

The figures selected for these observations were the table (Fig. 2) and the Mach truncated pyramid (Fig. 3), already used in previous experiments, the Beaunis cubes (Fig. 11) and the wedge depicted in Fig. 12. The observers were N. C. and C. S., who had taken part in all the experiments hitherto described. As the reader is already familiar with the nature of the numerical records obtained from this kind of experiment, and as the results were, almost without exception, very clear and unequivocal, I refrain from giving tables, and will merely indicate in a few words the principal conclusions to be drawn from these observations. With the wedge and the Beaunis cubes the results obtained from both subjects were very similar, if we make allowance for the usual individual differences in the power of voluntarily controlling the attention manifested by these subjects. With the wedge figure it was

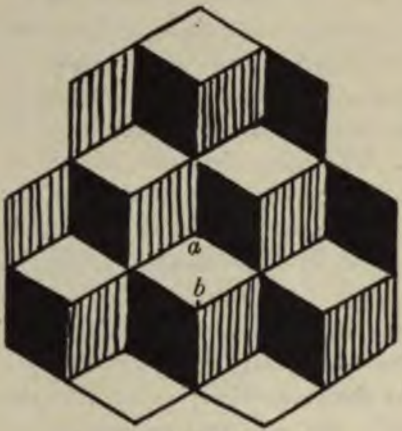

FIG. 11.

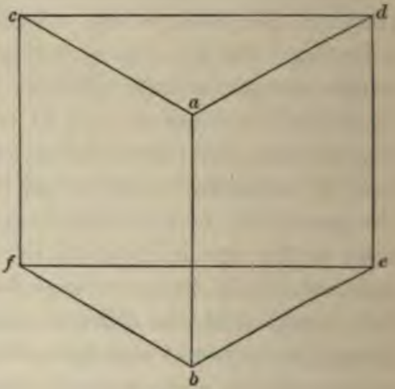

FIG. 12.

found that when attention was concentrated upon the line *ab* and its immediate neighbourhood, the thin end of the wedge was seen pointing towards the observer; when attention was strictly confined to the line *ab* itself, there was a tendency for all perspective to be lost, since the more peripheral parts of the figure were often not sufficiently in consciousness for the figure to be seen as a whole at all. When either of the horizontal lines was attended to, on the other hand, the wedge was always seen with its flat end toward the observer. The same was the case when the whole of the rectangular parallelogram *cdef* was the part attended to.

In the Beaunis cubes it was similarly found that attention to the point *a* caused the cubes to be seen from below, while attention to the point *b* caused them to be seen from above. In the case of the truncated pyramid, when attention was given to the outer square, the figure was seen as 'chamber'; when attention was given to the inner square, it appeared as 'boss.' In the latter case, however, a certain amount of attention had at the same time to be directed towards the outer square, as, when the attention given to this part of the figure fell below a certain minimum, the whole figure tended to become flat, the outer parts apparently not being sufficiently clearly in consciousness to become incorporated in any perspective whatever.

With the table figure, in the case of observer N. C., attention to the line *cd* brought out the 'bottom visible' perspective, attention to the line *eb* the 'top visible' perspective.

At the time these observations were made, observer C. S. had a very strong tendency to see the table in the 'top visible' perspective. As a consequence of this, it was easy for him to produce 'top visible' and maintain it almost uninterruptedly by attending to the line *eb*; attention to the line *cd* failed, however, to produce a predominance of the 'bottom visible' perspective, the legs of the table usually refusing to turn and obstinately continuing to appear as in 'top visible.' It was found, however, that the desired 'bottom visible' could be produced with comparative ease when attention was turned not upon the line *cd*, but upon the upper pair of legs themselves and particularly upon the left-hand member of the pair (*ac* in Fig. 2). The cause of this is probably to be found in that mutual inhibition of fixation and attention to which we referred above; for, as soon as the fixation point in the centre of the figure was removed and the subject was allowed to fixate some part of the line *cd*—*i.e.* to fixate and attend to the same part of the figure—'bottom visible' could be maintained during a considerable portion of the observation. Such mutual inhibition of attention and fixation is probably stronger when the parts of the figure attended to and fixated respectively are near one another than when they are more remote. This is indicated not only by the above results with the table (attention to leg *ac* more effective than attention to line *cd*) but also by some special observations, made with and without fixation on the truncated pyramid, which showed that fixation of a point in the middle of the figure detracted considerably from the efficacy of attention to the inner square, but had no such effect when attention was given to the outer square. These results are very possibly due to the fact that when the fixation point is abolished, eye movements frequently take place to the neighbourhood of the inner square, so that fixation and attention coincide during part of the time, and the inhibition between them is lessened; whereas the eye movements seldom extend as far as the outer square, so that the inhibition is the same as regards this part of the figure, whether a fixation point be present or not.

It will thus be seen that all our observations with these four additional figures are in close agreement with the results of the experiments upon the prism figure. We have then good ground for assuming that the general conclusion as to the dependence of the perspective upon the direction of the attention in that figure admits of application to all illusions of reversible perspective[1].

This conclusion, in fact, seems to afford an accurate expression of the general rule for the behaviour of such illusions, and in view of the importance which it thus comes to possess, it may perhaps be well to re-state it once more here. It is as follows: '*The nature of the perspective in which a reversible figure is seen at any moment is—at any rate, mainly—dependent upon the direction of the attention;* and this dependence of the perspective upon the attention is of such a kind that, *as regards all reversible parts of the figure, those parts which receive the more attention are seen in front of those which receive the less*

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[1] As our experiments were made upon a few subjects only, it remains of course possible (though unlikely) that this conclusion may not hold good for all observers. Since the value of our experiments depended largely upon the accuracy of the introspections, and since this accuracy can only be assured by taking a large number of records from well trained subjects, it was not found possible to repeat the experiments with a larger number of observers. Some attempt was indeed made to allow a number of junior students of Psychology to perform for themselves some of the experiments on fixation and attention reported above (Section V). But the results were not numerous enough to bring out clearly any individual differences as regards the dependence of the perspective upon the direction of attention, if such existed. Such as they were, however, these results on the whole agreed closely with those obtained from the practised observers.

attention...' The word 'attention,' as was indicated above, is here used as equivalent to 'clearness in consciousness[1].'

It may perhaps seem strange that such a simple empirical rule, if it really does successfully account for by far the greater part of the phenomena connected with illusions of reversible perspective, should not have been more generally recognised by those who have written on the subject, especially in view of the great amount of consideration which has, at one time or another, been given to these illusions. The general failure to attribute to the direction of the attention anything approaching the importance which, judging from our own experiments, it seems in reality to possess, may perhaps be partly accounted for on the three following grounds:

(1) The obscurity of the word 'attention,' which has notoriously been given many different shades of meaning by different writers. Even those who do attribute some importance to attention in connexion with the present subject have, as a rule, failed to define at all precisely the sense in which they were using the word, with the result that the expression has either been too vague to be of much service, or has given rise to direct misunderstandings on the part of subsequent investigators[2].

(2) The importance which, since the work of Wundt, has been attributed to fixation and eye movements. This has probably tended to divert investigators from the importance of the attention, since there is no doubt that the two factors work, for the most part, in the same direction, that point of the figure which is fixated usually

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[1] In the light of this conclusion we may perhaps explain some results of the earlier experiments with two figures seen simultaneously, described on p. 363. Thus it was found that the two prisms had longer periods of opposite phase than the two tables. In view of the fact that in these experiments no special instructions were given as to the direction of the attention, and that the point fixated lay midway between the two figures, we should naturally expect to find that the maximum of attention was frequently given to those parts of the figures which lay nearest to the fixation point. In the case of the prism this would, according to the above rule, always tend to produce a condition of opposite phase. With the tables we should not expect to find any such marked tendency, since here a distribution of the attention upon quite neighbouring parts of the two figures (*e.g.* upon *a* and *d* in Fig. 2) would result in the figures appearing in the same phase.

Although this seems a probable explanation of the difference in the results obtained with the prism and the table respectively, it must be remembered that the *general* tendency exhibited throughout the experiments with two figures seen simultaneously—viz. for the figures to appear in the same phase—shows that attention, under these circumstances, is usually given to corresponding rather than to neighbouring parts of the two figures.

[2] For the meaning of the word in the present paper see footnote, p. 376.

also receiving the most attention. It is indeed an important consideration in favour of our view, that it is consistent with the general validity of Wundt's rule as regards the relation between fixation and perspective, while at the same time (since fixation and attention do not *invariably* go together) it affords an explanation of those exceptions to the rule, which have been, among recent authors, generally admitted to occur.

(3) The difficulty of investigating the effect of the direction of attention. Partly on account of its usual agreement with fixation, partly for other reasons, it has not been easy to isolate the direction of the attention as a separate factor of the experimental conditions. It was only during the course of a long and laborious series of observations that its importance gradually became manifest in our own experiments. Further, all evidence as regards the direction of attention must, it would seem, in the last resort depend upon introspection, and introspection in this matter is by no means easy. It is not such a simple task as it might at first appear, to evaluate with any degree of accuracy the relative amount of attention given to the different parts of a figure at a given moment, and to observe in what way and to what extent the changes of perspective correspond to changes of attention. For this reason it is only by obtaining consistent records from a long series of observations with well-practised subjects, that reliable evidence can be procured.

It is of course true that, in spite of these difficulties, the influence of attention has been frequently noticed and referred to by previous investigators. They have, however, for the most part, regarded it as a factor of secondary importance only. Among the authors whose works are known to the present writer, there is nevertheless one whose views seem to be in close agreement with those we have been led to adopt as the result of the experiments here described. As the result of pure introspection upon a variety of figures, von Aster[1] seems to have come to the same conclusion as regards the influence of the direction of the attention, as that to which we have ourselves arrived as the result of introspections carried out under a variety of experimental conditions. Our own results, in fact, support the views of this investigator in a remarkable manner.

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[1] "Beiträge zur Psychologie der Raumwahrnehmung," *Ztsch. f. Psychol.* 1906, XLIII. 161.

VII.

We have now indicated sufficiently clearly the conclusions which seem to arise from the experiments here described. It will perhaps be of interest to indicate the nature of some of the further problems which seem to arise from these conclusions.

There is, in the first place, the genetic problem as to how the direction of the attention came to have the determining influence upon the perspective of a reversible figure. Wundt's hypothesis to account for the supposed influence of eye movements and fixation—that in viewing solid objects we normally direct the line of regard to a near point, and from thence explore more distant points as may be necessary, and that there has thus arisen an intimate association between the point fixated and the point from which an eye movement starts on the one hand and apparent nearness on the other—would probably admit of re-statement in terms of attention instead of fixation and has no doubt much to recommend it. In the absence of any experimental proof, however, it cannot at the present moment be accepted as anything more than an interesting suggestion.

Another closely allied problem is the question of the general place of the direction of attention as a factor in space perception. Our observations show that in illusions of reversible perspective drawn upon a plane surface, the direction of the attention is frequently the sole—or at any rate by far the most important—condition determining in which of two or more possible perspectives the figure shall appear. But it is obvious that with the majority of *solid* objects, a mere change in the direction of attention is no longer capable of producing such far-reaching changes in the apparent spatial relations of the object. It is known, however, that with certain objects occupying three-dimensional space, reversals of perspective can be observed exactly similar to those obtained with drawings such as those we have here used. This is particularly the case with real models made to resemble these drawings[1]. It would be an interesting subject of investigation to discover what is the *rôle* of the direction of the attention in the space perception of three-dimensional objects, under what conditions and to what extent it influences our perception of these objects in the same way as it does that of our reversible drawings, whether it exists as a factor of varying importance in all our spatial perceptions, or whether it is called forth only under particular circumstances[1].

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[1] See especially Burmester, "Theorie der geometrisch-optischen Gestalttäuschungen," *Ztsch. f. Psychol.* 1906, XLI. 321; 1908, L. 219.

A third interesting problem, arising from our view of the importance of the direction of the attention in illusions of reversal perspective, is concerned with the nature of this attention itself. In this connexion I will venture to conclude with a suggestion which, while no doubt requiring much further elaboration in the way of details, seems in its main outlines to harmonize well with the principal facts brought out course of this investigation. In a recent paper[1] much evidence ought forward in favour of the view that the energy concerned in ntal performance is largely derived from a single common fund, is drawn upon for every task the individual undertakes. If we cept this hypothesis, it would seem not unreasonable to suppose the 'clearness' of any particular content of consciousness, which most prominent feature of what we have here described as ion, is the result of the concentration of this 'general intellective y' upon that content; or, to express the same idea in physiological (and there seems every reason to believe that this common fund ergy is physiological, as well as perhaps also psychical in nature), the 'clearness' of any mental content is due to the concentration und of nervous energy derived from the whole brain (or at any rate the whole cerebrum) upon the physical substrate of that content. take a particular instance, in the case of our prism (Fig. 1), the tion of the attention to, say, the right-hand square (*i.e.* the greater arness' of the right-hand square in consciousness) is, we may sup-, a consequence of the direction of the general nervous energy upon nervous elements corresponding to the perception of the right-hand uare. In so far as the whole figure is perceived, part of the total ergy is no doubt going also to the other parts of the figure, but the oportion of this energy going to these latter is smaller than that ing to the right-hand square.

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[1] A few observations in this direction were made in connexion with the present investigation. Two wire models of the prism (Fig. 1) were prepared, in one of which the two ends were made of the same size, while in the other some attempt was made to allow for the effect of perspective by making that end of the model which was placed nearer the observer smaller than that which was remote from the observer. When looked at from a suitable distance this latter model gave a retinal image which was nearly (the construction of the model was not quite accurate for this purpose) the same as that given by our drawing of the prism figure; while with the other model the image was of course different, in so far as the effects of perspective were not counteracted. By comparing similar observations made upon these two models we were thus able to compare the influence of a simple perspective effect with that of the direction of the attention. It was found that the perspective effect (as produced by the model in which the perspective had not been corrected) did not in any way appreciably hinder the reversals occasioned by the movements of attention, since it could easily be 'allowed for' by an alteration in the apparent relative size of the two ends of the model. When the end which was in reality remote from the observer appeared the nearer, it seemed to be a good deal smaller than the apparently remote (in reality the nearer) end, whereas when there was no such reversal, the two ends appeared (as they really were) to be of the same size. No such apparent change in the relative size of the two ends occurred with the model in which the perspective had been corrected, but otherwise the behaviour of the two models was practically identical. [A similar but less marked effect, however, could sometimes be observed with this model also and with the drawn figures. In this case the retinal image of the two ends was the same size, whereas in the case of a real object whose ends were the same size, the image of the more distant end should be smaller. This was 'allowed for' in the interpretation of the image, with the result that the apparently more distant end—whichever this happened to be—appeared slightly larger than that which was apparently nearer.]

By observing the models with one eye and two eyes alternately, it was similarly possible to compare the influence of attention with that of disparation. Unlike the perspective effect, it was found that disparation offered considerable difficulties in the way of reversal, in so far as it was much easier to obtain reversals with one eye than with two. But while reversals could not be obtained so frequently with two eyes or maintained so long, the reversed perspective itself, when present, was quite as clear and unmistakable as when observed with one eye. It is interesting also to note that the individual peculiarities of the observers as regards the power of voluntary control over the perspective by means of the attention were maintained under these circumstances. Thus N. C. was able, by means of strong concentration of attention upon the remote end, to resist the effect of disparation more successfully than was C. S. Here then we have a case, probably worthy of further investigation, in which the effect of the direction of the attention seems to be struggling for the mastery against another important factor of space perception.

In order to account for the appearance of the 'right forward' erspective under these conditions, we must suppose further:

(1) That the perception of the right-hand square is more closely ssociated with the appearance of 'right forward' than it is with the ppearance of 'right backward' (or, what is practically the same thing, left forward')[2].

(2) That, owing to this association, the energy directed to the right-hand square more readily overflows into the 'right forward' than into the 'left forward' system. In this way the observed coincidence of 'right forward' perspective with 'clearness' in consciousness of the right-hand square can be readily accounted for, as can also the coincidence of 'left forward' perspective with 'clearness' of the left-hand

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[1] Bernard Hart and C. Spearman, "General Ability, its Existence, and Nature," this *Journal*, 1912, v. 51.

[2] As regards the *existence* of such an association there can, I think, be little doubt. To account for its *origin* we need, however, some such hypothesis as that of Wundt referred to above.

square. The absence of perspective which seems usually to accompany the even distribution of attention over the whole figure (p. 377) is, we may suppose, due to the mutual inhibition of the two perspective systems, owing to an approximately equal amount of energy overflowing into both simultaneously; while the similar absence of perspective observed when attention is strongly concentrated on the centre of the figure (p. 378) is possibly the result of an insufficient overflow of energy into either system. The observed very high correlation between attention to any part and the 'forward' appearance of that part seems to indicate that the association between the perception of that part and its 'forward' appearance is *very much* stronger than is the association between the same perception and its 'backward' appearance. There can, however, be little doubt as to the existence of a weak association of the latter sort, and in virtue of this association there should be little difficulty in accounting also for such exceptions to the general rule as may occasionally be observed to occur.

If we provisionally accept this hypothesis as affording a plausible explanation of the main problem of the nature of the attention involved in illusions of reversible perspective, there still remains a number of allied questions of very considerable interest. To mention a few of the most important: Why cannot the attention be steadily maintained upon any one part of the figure, so as to maintain the same perspective indefinitely? To what are due the large individual differences in the power of thus steadily maintaining the attention? What, if any, are the relations of the movements of attention which accompany the reversals of perspective to the so-called 'fluctuations of attention'? I hope to be able to enter upon a consideration of some of these questions in a future paper.

Summary of Principal Results and Conclusions.

Two reversible figures, seen simultaneously, fluctuate to some extent independently of one another. This is shown to take place with (*a*) two identical juxtaposed figures, (*b*) two different juxtaposed figures, (*c*) two different superposed figures. These results afford strong evidence against (1) any theory which attributes the reversals of perspective to any physiological cause (*e.g.* the Traube-Hering waves) affecting the whole of the central nervous system at the same time, (2) the usual form of the theory which attributes the reversals to eye movements.

Experiments with a series of drawings designed to make the appearance of one perspective progressively more difficult show that complicating detail is of assistance in preventing reversal when it is attended to; but that reversals may persist, even with very considerable complication, so long as attention is concentrated on the main outlines of the figure.

Observations of the prism figure with different attitudes show that the direction of the attention is a factor of the greatest importance in the reversals of perspective. The general rule is that that part of the figure which receives the more attention is seen nearer the observer. There exist considerable individual differences in the power of controlling the perspective.

Experiments on fixation with the help of after-images show that these results cannot be due to eye movements, though strict fixation and attention to neighbouring points probably inhibit one another to some extent.

The results obtained with the prism figure are confirmed on other figures.

Owing to various causes, the influence of attention has been unduly neglected by most previous investigators.

The important part played by attention in illusions of reversible perspective gives rise to several further interesting problems.

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AN INQUIRY INTO THE BEST FORM OF THE METHOD OF SERIAL GROUPS.

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(1) *Analysis of the Method of Serial Groups and a general consideration of its advantages and disadvantages, leading to the enunciation of two definite problems.*
(2) *A first examination of these problems:*
  (*a*) *What is the best proportion of correct answers to adopt for the critical group?*
  (*b*) *What is the best size of the group?*
(3) *A further examination of these questions by the application of general equations of the Group Process to a particular set of data.*
(4) *Direct application of the various Serial Group Methods to these and other data.*
(5) *Conclusions.*
Appendices I—V.

1. *Analysis of the Method.*

THE various methods[1] of measuring thresholds differ from one another (*a*) in their convenience under different circumstances, (*b*) in their psychological effect by suggestion or otherwise on the subject (and probably also on the experimenter), and (*c*) in the mathematical processes which they adopt, either by custom, or of necessity from the form in which the data are collected. The Method of Serial Groups may be defined as follows[2], using for convenience the nomenclature of the cutaneous spatial threshold.

Commencing at a stimulus value at which practically all the answers are two, each stimulus value is presented to the subject a number of times in succession, usually five or ten, interspersed with a (usually the same) number of 'catches' in which only one point of the aesthesiometer is applied, the whole forming a group. Successive groups are tried with decreasing values of the stimulus until a group is reached in which only eighty per cent. or less than eighty per cent. of the answers to double touches are 'two.' The stimulus value at which this happens is noted, the descent is stopped, and the whole process is then repeated a number of times giving a number of observations of the eighty per cent. point, the arithmetic mean of which is then taken. It is customary to vary the starting-point and the size of the steps. Ascents can also be made.

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[1] See this *Journal*, 1912, v. 208, for terms, method and process.

[2] See Appendix I.

The chief advantages of the method arise from the arrangement in groups. The most important point is that this device enables 'catches' to be systematically introduced, thus lessening the effect of suggestion. Less important advantages are that observations can be more quickly and more regularly taken in groups than separately. None of these advantages have anything to do with the fact that the groups are serial, they are also present in the Method of Non-Consecutive Groups advocated elsewhere by the writer[1].

The arrangement of the groups into serial order, either descending or ascending, probably arose by analogy with the Method of Minimal Changes. The serial order is to this extent psychologically disadvantageous, that it may be detected by the subject whereby the influence of suggestion is reintroduced, though kept in check by the presence of 'catches.' In experimenting on children or primitive people, however, the experimenter will find it easier to explain the method if the serial order is used and can also by its help arouse emulation more easily, if he wishes to do so. Its advantage elsewhere lies in the fact that a descent can be stopped as soon as the critical point is reached, and thus time is saved. If the descent is not stopped at this point but continued right through then the writer has elsewhere shown[2] that there is no need for it to be a descent at all, the groups might as well be non-consecutive.

By thus stopping its descents at the critical points, however, the Method of Serial Groups is forced into accepting a certain mathematical process for the reduction of its data, namely it takes the arithmetic mean of the critical points as the threshold. On the other hand, the Method of Non-Consecutive Groups can adopt any process whatever for the reduction of its data. This process which the Method of Serial Groups is forced to accept, and which we may call the Group Process, is simple and involves no laborious calculation. Its accuracy depends *inter alia* on the size of the group. One of the objects of this paper is to examine this point theoretically and to determine what is the best size of group, and by what other means the Group Process can be made more accurate.

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[1] This *Journal*, 1912, v. 204, 214.

[2] This *Journal*, 1912, v. 213.

Finally, the Method of Serial Groups adopts eighty per cent. of correct answers as the measure of the least perceptible difference. This is a purely conventional choice[1] and rested probably on an imperfect notion of the meaning of a threshold. The experimenter set himself to find the least perceptible difference and hence felt that he could not descend to a 50 °/₀ of right answers. Clearly 100 °/₀ was too high, and he hence took an approximate mean as a useful conventional rough value. The actual choice of 80 °/₀ perhaps arose in McDougall's[2] case from the fact that he took five double touches for one group. Stratton[2], who took ten readings for one group, also could not get the exact quartile and so chose 80 °/₀. Neither of these writers says what percentage he would take for ascents, but Myers in his measurement of the Smallest Perceptible Tone Difference among Murray Islanders[3] used 80 °/₀ for ascents as well as descents, and gives this in his description of the Method, which is probably the standard for England, in his text-book, p. 197.

This adoption of the 80 °/₀ point is at once open to the objection that experiments thus made cannot be compared[4] with those made by other experimenters who give the 50 °/₀ point. Even direct comparison of 80 °/₀ measurements may be misleading since of two individuals or classes one may have a higher 80 °/₀ point but a lower 50 °/₀ point than the other, and it is clear nowadays that it is the central value which we wish to compare. Only between 80 °/₀ measurements which have approximately the same scatter can direct comparison be safe. From this point of view it would seem advisable to abolish 80 °/₀ and use only 50 °/₀. There are, however, further questions involved.

It has already been pointed out that the Method of Serial Groups saves time by not continuing a descent beyond a critical point. Now since the 80 °/₀ point comes in a descent before the 50 °/₀ point, each descent will be shorter when the former value is being used, and therefore more observations can be made of the 80 °/₀ point than of the 50 °/₀ point in the same time, when descents are being used.

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[1] See Stratton, quoted in Appendix I.

[2] See Appendix I.

[3] *Rep. Cambridge Anthrop. Expedition to Torres Straits*, Cambridge, 1903, II. 155—168.

[4] See Appendix V, for a limitation of this statement.

This will not in itself as a rule be an advantage unless the mean value derived from the numerous 80 °/₀ values is of greater accuracy than that derived from the smaller number of 50 °/₀ values. That is to say, the *precision* of each 80 °/₀ value may be so much less than the *precision* of each 50 °/₀ value that the greater number of the former values cannot bring with it a sufficient compensation. This is the other point to be examined theoretically in what follows.

Even if it should prove that in the same time the 80 °/₀ can be measured more accurately than the 50 °/₀ point yet the general objections to the 80 °/₀ point given above would still hold and it would be necessary to show that the gain in accuracy is sufficient to outweigh them. If, however, the 80 °/₀ point proves to be not more quickly measured with accuracy than the 50 °/₀ point, then all reason for its adoption disappears except perhaps in the following case.

If the measurements which are being made are for the purpose of obtaining a racial average value, then it is of primary importance that as many members of the population be measured as possible in the time. The 80 °/₀ point will enable more subjects to be examined than will the 50 °/₀ point, and if the accuracy of the mean of these is as great or nearly as great then the experimenter may consider the 80 °/₀ point justified, even bearing in mind the general objections urged above. The fact that it has been used on primitive people in the past would no doubt also influence him in his decision.

If ascents as well as descents be used however, and 80 °/₀ taken for both, then there can be no saving of time and the 80 °/₀ point is not advisable. For the ascents, which must start from a point where practically all the answers are one[1], will by their length waste all the time saved by short descents. It might, however, conceivably be an advantage to use 80 °/₀ for descents and 20 °/₀ for ascents[2].

We now proceed to consider, from the mathematical point of view, the two questions raised above.

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[1] The average of a number of observations is a function of the starting-point and if this starting-point be elsewhere than at a point where all or practically all the answers are two or one it becomes impossible to say what is being measured. Ascents in aesthesiometric work are often thus of doubtful value since even at zero p often has a fairly large value.

[2] Cf. this *Journal*, 1912, v. 240.

(*a*) Is the accuracy of the mean of a number of 80 °/₀ points greater or less than the accuracy of the mean of that number of 50 °/₀ points which could be measured in the same time?

(*b*) What is the best size of the group?

Descents only will be considered. While desiring to avoid the use of mathematical symbols in the text as much as possible it will make statements more concise if the following abbreviations are used:

g = No. of double touches in a group.

t = No. of answers *two* adopted for the critical group (that is, in the ordinary case $t = \cdot 8g$).

p_κ = probability of an answer *two* being given at r_κ cms.

$q_\kappa = 1 - p_\kappa$.

2 (*a*). *What is the best proportion of correct answers to adopt for the critical group?*

On general grounds it is to be expected that a method which makes observations in the neighbourhood of 80 °/₀ will be *for the same number of descents* less accurate than one making observations in the neighbourhood of 50 °/₀[1]. If the method of least squares is used in solving an inconsistent equation system, then the weight of an observation equation at r_κ is proportional to

$$\frac{\left(\frac{dp}{dr}\right)_\kappa^2}{p_\kappa q_\kappa},$$

and all the hypotheses which have been put forward as to the form of the psychometric function agree in making this expression reach a maximum at 50 °/₀. It is possible, however, to suggest forms of the psychometric function in which this would not be the case; for example, the form

$$hr = \pm \sin^{-1}(1 - 2p)$$

(in which h is an arbitrary parameter) would give equal weight to all observations[2].

We are, therefore, brought face to face with the fact that the question of the best form for a group process is bound up with the question of the best fitting psychometric function. From our knowledge of such functions as have been tried, we can say however that probably 80 °/₀ will give less accurate results than 50 °/₀ *for the same number of descents.*

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[1] See Urban, *Arch. f. d. ges. Psychol.* 1909, xvi. 370.

[2] After this paragraph had been written, this same function was suggested by Professor Urban, in a letter to the writer, as one which might be tested on some empirical material.

The descents will, however, be on the average shorter for 80 °/₀ than for 50 °/₀ and the real question is whether for the same number of separate observations or for the same time the ratio of the number of 80 °/₀ critical groups to the number of 50 °/₀ critical groups will be sufficiently large to outweigh the greater accuracy of each 50 °/₀ critical group. This question can be most readily appreciated by a consideration of Fig. 1. The upper curve shows the course of a psychometric

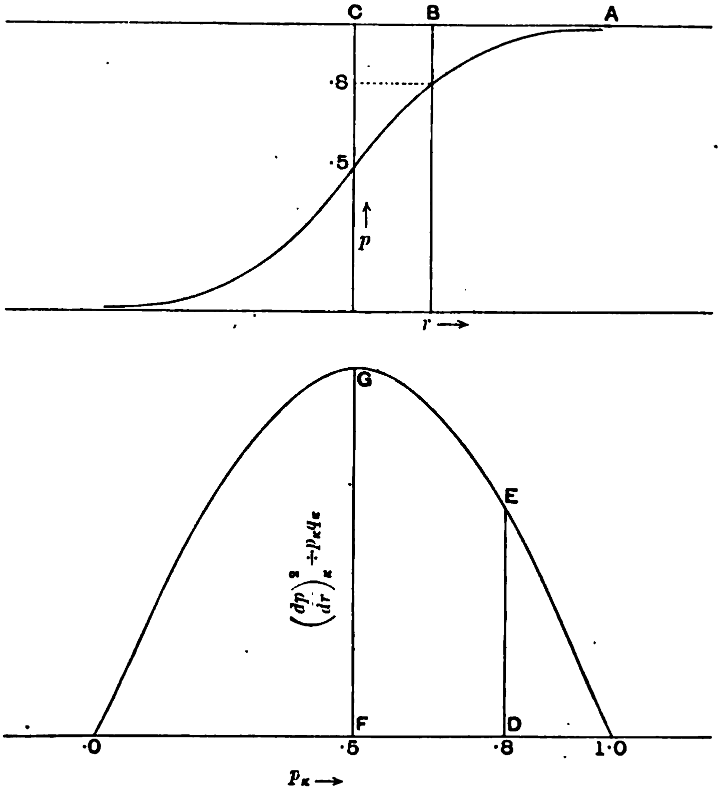

FIG. 1.

function, the lower curve shows the course of the weighting function given above. Since the usual forms of psychometric function only give $p = 1$ for $r = \infty$ a point A must be chosen at which p is to be regarded as practically equal to unity. The average descent for 80 °/₀ is represented then by AB, the average descent for 50 °/₀ by AC. In any given time the number of 80 °/₀ descents will be greater than the number of 50 °/₀ descents in the ratio

$$\frac{AC}{AB}.$$

Each 80 °/₀ critical group has, however, a less weight than each critical group in the ratio

$$\frac{DE}{FG}.$$

The adoption of 80 °/₀ will, therefore, only have given a greater accuracy if

$$\sqrt{\frac{AC}{AB}\cdot\frac{DE}{FG}} > 1.$$

In the case of any given psychometric function this ratio can be calculated when A has been arbitrarily chosen. For the Gauss integral if we assume $p = \cdot 99$ as being practical certainty we can take the values from published tables, *e.g.* Fechner's Fundamental Table and Urban's Table of Weights and we find

$$\sqrt{\frac{1\cdot 6450}{1\cdot 0499}\times \cdot 770} = 1\cdot 1.$$

In this case, therefore, these considerations lend support to the belief that the 80 °/₀ point can in the same time be measured with as great or perhaps slightly greater accuracy than can the 50 °/₀ point, by the method of Descending Serial Groups. Any displacement of the point A in Fig. 1 towards higher values of p, that is to say, any tendency on the part of the experimenter to begin too far above the threshold, will reduce the advantage of the 80 °/₀ point in this respect. We leave now for a while the question of what percentage to adopt, and turn to the other question, what size of group is the best.

2 (*b*). *What is the best size of the group?*

Considerable light is thrown on any mathematical question by the consideration of extreme forms. The extreme numbers available for the size of a group are on the one hand unity (giving the ordinary Method of Minimal Changes, no grouping at all), and on the other hand all the observations at each stimulus value. If we take the latter case we have only one determination of the critical group.

Let us suppose that time permits us to take an enormous number of separate judgments at each stimulus value, so that the proportion of answers *two* at each is not affected by sampling: and let us suppose we are measuring the 80 °/₀ point and are taking all the judgments at each stimulus value as one group. We then take the critical point in a descent as the stimulus value next below 80 °/₀ and in an ascent as the stimulus value next above 80 °/₀. Unless we have been extraordinarily lucky in our choice of stimulus values neither of these will be at the 80 °/₀ point, nor need their arithmetic mean be nearer than the best of the two: with such a large group therefore, there is not merely a probable but an almost certain error.

With smaller groups this difficulty (of being unlucky in choice of stimulus values) is not removed, but it becomes possible to vary the stimulus values from one descent to another, as Urban has already recommended in the Method of Minimal Changes[1]. This possibility is greater, the smaller the group, and hence we are led to anticipate that small groups are more advantageous than large ones. If we stick to 80 °/₀ the minimum group is of course five.

Further, in making an examination of a racial feature, a limit is put on the size of group used, by our desire to examine many subjects. In such a case the change of subject would have the same effect as a change of the stimulus values and the latter would not be necessary. Other disadvantages of very large groups are of a psychological nature, for changes in attention, fatigue, &c. would not be spread over many descents. We are concerned here, however, with the size of group best from a mathematical point of view only.

We saw above that if only one set of stimulus values is used the Method of Serial Groups will not, excepting by an extraordinary lucky choice of these stimulus values, measure the exact point it is trying to detect (say the 80 °/₀ point) but one differing from this, even if both ascents and descents are taken. We must notice further that, for the same set of stimulus values, what is actually measured depends on the size of the group, for the probability of the subject returning four correct answers out of five is not the same thing as the probability of his returning eight out of ten. It becomes necessary, therefore, to examine the whole question in more detail and to set up general equations for a general Method of Serial Groups. By applying these equations to particular sets of data we shall obtain further light on the two questions we are considering: and the results obtained will be confirmed by the direct observation of the data.

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[1] F. M. Urban, *Arch. f. d. ges. Psychol.* 1909, xv. 300 ff.

3. *Application of general equations to a particular set of data.*

These equations are deduced in Appendix II. The threshold $\mathfrak{T}'$, the arithmetic mean of the critical groups observed in a number of descents, depends on the probabilities p_r that the subject will return an answer *two* at the various stimulus values r. If the real values of p_r were known a most probable value of $\mathfrak{T}'$ could be definitely ascertained by equation II and the value actually observed would tend towards this calculated value as the experiment was prolonged.

But not the real but only the observed values of p are known; these are subject to probable errors which can be written down, and thence the probable error $\mathfrak{F}'$ of the calculated value $\mathfrak{T}'$ can be found by equation IV. The serial group threshold and its probable error can thus on the one hand be calculated from the observed values of p. These calculated values of threshold and probable error depend (a) on the number g of double touches in a group, (b) on the number t of answers *two* taken as indicating a critical group, and (c) on the number n of steps: and we can strictly speaking only say what are the best values for these quantities if we know beforehand what number of answers *two* and *one* the subject is going to give at each stimulus value. If we had this knowledge we should of course not need to experiment at all.

But in practice we do as a matter of fact have a fair working idea of how the answers will run. If we can predict the approximate points at which the subject will give answers nearly all *twos* and nearly all *ones* respectively (a preliminary trial would help), and if we know a psychometric function which is a good fit for the results of similar experiments we can if we have sufficient patience and mathematical skill find from Appendix IV equation V what are the best values for g, t and n.

Put mathematically the problem is to adopt a function connecting p and r, introduce it into equations I—IV and obtain from V an equation system between g, t, n, on the one hand and the parameter or parameters of the function on the other. This problem the writer has not yet seriously attempted. Instead it is proposed to apply the equations to some data already known, and to see what values of g and t would have been best in this case had the Method of Serial Groups been adopted. For this purpose the measurements of the cutaneous spatial threshold on the right forearm on subject No. 6, described by the writer in this Journal (1912, v. 214–5, Table VI), have been used.

In these measurements fifty[1] double touches were submitted at every half centimetre up to five cms. They were not made by the Method of Serial Groups but by the Method of Non-Consecutive Groups. It must be emphasised that this does not affect the argument adversely in any way, for we are not here concerned with the experimental method by which data are collected, *qua* method, but with the mathematical process adopted in making the subsequent calculations. Confusion is particularly likely to creep in as to this point with the Method of Serial Groups because this method forces us to use the Group Process[2] and the method and process become indistinguishable (much as mass and weight are confused by students in physics). We are examining the Group Process, not the method, and we propose to use various forms of the process on data collected by another method altogether. The writer has already shown that this is legitimate[3]. The advantage thereby gained is that the data are throughout the same, only the process is changed. Moreover, since the same number of judgments were made at each half centimetre, the calculations are considerably simplified.

The results of the calculations are given in Table I, the first two columns of which give the data from which the whole of the rest of the Table is calculated. The serial group thresholds are calculated for various sizes of group and various values for the number of correct answers indicating a critical group. The case $g=1$ is of course the Method of Minimal Changes. The columns headed 'McDougall' demand further explanation.

McDougall[4] employed the device of doubling the critical group. That is to say, he descended until he found the critical group, but did not record it unless on repeating that group he got a similar result. Details, and the necessary changes in the mathematical formulae, are indicated in Appendix III. He used groups of five and took the 80 °/$_\circ$ point.

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[1] At 0 cms. five hundred.

[2] See p. 400 *supra*.

[3] This *Journal*, 1912, v. p. 213, see also Appendix II to the present article, Equations I and II, from which the conclusion follows more generally.

[4] *Rep. Cambridge Anthrop. Exp. to Torres Straits*, Cambridge, 1903, II. p. 190.

The comparative accuracy of each process is represented by the numbers in the last row of the Table, calculated from the formula there given. These numbers are the reciprocals of the proportional probable errors, corrected so as to represent the comparative accuracy for the same number of individual judgments, that is for the same time[1].

TABLE I. *Values of the Threshold and its Probable Error as calculated from the values of p by various Processes, and the Comparative Accuracy of each Process.*

		$t=5$ $g=10$		$t=8$ $g=10$		$t=4$ $g=5$		McDougall		$t=0$ $g=1$	
r cms	p	$\mathfrak{P}'$	$10^3 \times 49\mathfrak{S}'$	$\mathfrak{P}'$	$49\mathfrak{S}'$	$\mathfrak{P}'$	$49\mathfrak{S}'$	$\mathfrak{P}'$	$49\mathfrak{S}'$	P'	$49S'$
0	·028	·00	·892	·00	·270	·00	28·2	·00	6·97	·00	28·3
0·5	·06	·00	·562	·00	·116	·00	12·6	·00	3·06	·01	12·6
1	·12	·01	·420	·00	·043	·00	5·2	·00	1·22	·04	5·4
1·5	·24	·22	·009	·00	·014	·00	2·1	·00	0·43	·16	2·4
2	·48	·47	·003	·02	·005	·01	1·0	·09	0·17	·22	1·9
2·5	·64	·26	·010	·26	·001	·12	0·4	·37	0·05	·24	2·2
3	·82	·03	·028	·36	·001	·22	0·3	·30	0·09	·15	4·8
3·5	·90	·00	·046	·23	·002	·25	0·3	·16	0·23	·09	9·1
4	·96	·00	·352	·05	·010	·14	0·9	·03	0·82	·04	24·1
4·5	·98	·00	·746	·02	·020	·08	2·3	·01	2·26	·02	49·0
5	·96	·00	·352	·06	·010	·18	0·8	·03	0·81	·04	24·0
Threshold $\mathfrak{T}'$		2·04		3·15		3·67		2·88		2·51	
Probable error $\mathfrak{F}'$		0·36		0·60		0·62		0·45		0·22	
No. of double touches[2] N		346		234		183		350		299	
Comparative accuracy[3] $\mathfrak{T}'/\mathfrak{F}'\sqrt{N}$		0·30		0·34		0·44		0·34		0·67	

The numbers in this Table are only given to two or three figures, but they were calculated to at least four figures and where necessary to more.

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[1] See Appendix IV.

[2] All the values in this Table are calculated solely from the p's and r's; this applies to N also. The formulae are given in Appendices II, III and IV. The numbers given for N are thus not actual numbers—in that case the first four would be multiples of five—but the most probable proportions for the various N's for the same number of judgment at 5 cms.

[3] See Appendix IV.

Before drawing the conclusions which seem obvious from Table I we will, however, proceed to examine the same data in another and more direct way.

4. *Direct Application of the various Serial Group Processes to these and other Data.*

The threshold, probable error, number of double touches, and comparative accuracy have been in Table I calculated from the *p*'s. They can be, however, directly observed from the original data which give the answers to the double touches, in columns under the different values of the stimulus, and in rows[1]. For $g = 1$ each row is taken separately and examined from right to left till the critical point is reached where the first wrong answer is given. For $g = 5\,(10)$ the first five (ten) rows are taken together, then the next five (ten) rows, and so on, and each group of rows examined from right to left till the critical point is found. For McDougall's Process the first set of five rows is taken for the first descent and the second set of five rows used for doubling. The second set is then used for the second descent and the third set for doubling and so on, the first set being used in doubling during the last descent.

A *sample* distribution of the critical groups for each process is thus found and the threshold is their arithmetic mean. The actual number of double touches required can be counted in each case beginning always at the right (it is assumed that each group begun is completed). The probable error $\mathfrak{F}'$ on this direct plan of marking can be found by using the formula

$$\mathfrak{F}' = \frac{\cdot 6745}{m}\sqrt{\frac{\Sigma\, r^2 a\,(m-a)}{m-1}},$$

where m = number of descents,

a = number of critical groups observed at r.

These observed values are put together in Table II. Complete agreement is not to be expected with Table I the values in which depend upon all the data, whereas Table II is only a sample. Other observed values could be got from the data by shuffling them.

The observed probable errors are, therefore, to be expected greater than the calculated, as is the case. Table II is a useful check against algebraical or arithmetical slips in calculating Table I.

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[1] Table VI, this *Journal*, 1912, v. 215, is sufficient except for the case $g = 1$.

TABLE II. *For comparison with Table I, being samples of the observed values obtained by direct application of the various Processes to the data upon which the p's of Table I are based.*

	$t=5$ $g=10$	$t=8$ $g=10$	$t=4$ $g=5$	McDougall	$t=0$ $g=1$
Threshold $\mathfrak{T}'$	2·20	3·30	3·50	2·50	2·36
Probable error $\mathfrak{F}'$	0·53	1·00	0·74	0·50	0·23
No. of double touches N	330	220	200	410	314
Comparative accuracy $\mathfrak{T}'/\mathfrak{F}'\sqrt{N}$	0·23	0·22	0·33	0·25	0·57

Before drawing any conclusions we must bear in mind that only one set of data has been examined and that the results obtained might conceivably only be true for this set, perhaps because of the particular choice of stimulus values, perhaps because of the particular way in which p here depends on r, perhaps because of the reversal at 5 cms. Against this might be urged that the reversal is slight, that calculations for ascents give similar values (the comparative accuracies for example of a five-tenths Serial Group Method and the Method of Minimal Changes come out at ·259 and ·737 respectively), and that as values closely approximating to $p = ·5$ and $p = ·8$ both actually occur the 80 $^\circ/_\circ$ point is probably on an equally favourable footing with the 50 $^\circ/_\circ$ point.

To bring additional conviction, however, we proceed to examine another set of data, contenting ourselves with the simpler direct method of Table II, and not facing the laborious calculations necessary for the production of Table I. And to emphasise that the problem here considered is a purely mathematical one, the data are not taken from any psychological experiment but from a ball-drawing experiment in chance. Let eleven urns be taken and labelled

0, ·5, 1, 1·5, 2, 2·5, 3, 3·5, 4, 4·5, 5 cms. respectively.

Let fifty balls be placed in each, some black and some white and let the number of white balls in each be

4, 8, 12, 18, 25, 32, 38, 42, 46, 48, 49 respectively.

These numbers are chosen because they agree as nearly as possible with the $\Phi(\gamma)$ or Gauss-integral hypothesis of the psychometric function.

With these urns, if we let a white ball mean an answer *two*, a black ball an answer *one*, we can imitate any psychophysical method. Using for example a four-fifths Serial Group Method, we should take five balls out of each urn in turn (beginning with the right-hand one, and replacing each ball after examining it) until we drew a black ball. If the urn at which this happened is labelled say 3 cms. we should note this and begin again.

In practice one pack of fifty suitably-marked postcards can be made to represent each urn in turn. With such a pack experiments were made by the various methods under consideration and as a further precaution care was taken that the number N of separate judgments (here cuttings of the pack) was as nearly as possible the same in each method. Table III shows the result.

TABLE III.

	$t=5$ $g=10$	$t=8$ $g=10$	$t=4$ $g=5$	McDougall	$t=0$ $g=1$
$\mathfrak{T}'$	1·90	3·39	3·74	3·23	2·67
$\mathfrak{I}'$	0·49	0·69	0·51	0·62	0·21
N	360	380	370	365	368
Comparative Accuracy	0·20	0·25	0·38	0·28	0·65

Finally before proceeding to answer the two questions which we proposed on p. 402 one more word of warning is necessary. It will be noticed that the three 80 °/$_0$ thresholds in any Table do not agree with each other, nor do the two 50 °/$_0$ thresholds, and moreover in the case of Table III where the actual 80 °/$_0$ and 50 °/$_0$ points are known, the numbers obtained in no case agree with the real points: and it might occur to the reader to judge of the accuracy of a method by the amount of agreement in Table III with the known values. But this would be wrong, for the extent of this agreement depends upon the accident of the choice of stimulus values which we have not changed in any way during the experiment. In using any of the methods in practice it is imperative to change the stimulus values continually[1] (or to change the subject frequently, in racial work): and it is more satisfactory to change the actual values of the stimuli, not merely to vary the size of the steps by omitting some stimuli, though this will do some good[1]. If this be done then in the limit the mean of a number of ascending and descending values obtained by a four-fifths method will agree with that obtained by an eight-tenths method, and with the actual 80 °/₀ point. And it has been pointed out above that with small groups such changes in the series of stimulus values can be more frequently carried out.

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[1] Cf. F. M. Urban on the Method of Minimal Changes, *Arch. f. d. ges. Psychol.* 1909, xv. 317: "Man stelle möglichst viele verschiedene Reihen von Vergleichsreizen her."

5. *Conclusions.*

Although the general case has not yet been solved, the cases examined indicate that the answers to the two questions proposed are probably

(*a*) the 80 °/₀ point can be measured rather more accurately than the 50 °/₀ point in the same time;

(*b*) the smaller the group the better.

The use of the 80 °/₀ point (for descents) enables a larger number of subjects to be examined in a given time, and the accuracy of the mean of these readings is slightly greater than the accuracy of the readings of the 50 °/₀ point which can be made in the same time. It will no doubt, therefore, continue to be used whenever it is of great importance that as many subjects as possible be examined: but whenever possible the 50 °/₀ point should be preferred so as to facilitate comparison with other experiments.

The answer to the question as to the best size of group shows that the simple Method of Minimal Changes is mathematically superior to any Method of Serial Groups. It must be repeated that this only refers to the Method of Serial Groups as such, in which descents or ascents are stopped as soon as the critical point is reached. If the descents are continued right through there is then no necessity for them to be descents at all, the groups can be taken in any order and the necessity of applying the Group Process disappears. To data collected by this method of Non-Consecutive Groups should be applied either a Constant Process assuming a psychometric function, or if it is desired to avoid laborious calculation, the Limiting Process, but not a Group Process.

Many cases will, however, arise in practice where time does not permit of a complete examination and for such cases (*e.g.* measurements on primitive people) the Method of Serial Groups has great experimental and psychological advantages. The question then arises (since large groups are at a mathematical disadvantage) as to how small the groups may be made without losing the experimental advantages. The smallest groups which have been used were fives, but the writer suggests four (plus of course about four catches) as a still better number for the following reasons,

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[1] Further experiments were made with the marked cards, omitting various steps, with results similar to those described, as far as comparative accuracy of methods is concerned.

(*a*) the mathematical accuracy is greater[1];

(*b*) the 50 °/₀ point can be taken since four is an even number;

(*c*) the upper quartile 75 °/₀ in descents and the lower quartile 25 °/₀ in ascents can be noted *en passant*.

This last point is an advantage in itself as a measure of the sharpness of the threshold; and the upper quartile can be directly compared with the old 80 °/₀ measurements, while in future if in any measurements it is felt that circumstances justify a departure from 50 °/₀, then 75 °/₀ for descents and 25 °/₀ for ascents should be chosen[2].

APPENDIX I

DEFINITIONS OF THE METHOD OF SERIAL GROUPS.

McDougall[3], who was the first to use the Method, used groups of five and doubled the critical groups as explained on page 407. Further, when in a descent 80 °/₀ did not actually occur he took the mean of the neighbouring values. He ignored the answers to single touches except as a check on the subject's good faith.

G. M. Stratton who invented the Method independently and was the first to describe and name it[4] used groups of five tests and five catches and says, 'In my own computations that group has been taken as giving the threshold beyond which less than eight out of ten judgments are right,' that is to say, mistakes *either in the real tests or the catches* were taken as indicating the threshold. This practice does not seem to have been followed up. About the proportion eight-tenths Stratton says, 'a detail like this, as well as the exact number of experiments that may best form a group, might well be considered as subject to revision in the light of further experience and not as an essential part of the method.' This is the problem of the present paper which Stratton's words might well have suggested: but it was as a matter of fact completed before I had read his article.

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[1] This was tested with the cards. The comparative accuracy for comparison with the numbers in Table III came out as ·34 for 50 °/₀ and ·45 for 75 °/₀.

[2] After completing this article, while glancing through Vol. I. of *Biometrika*, I chanced to notice certain formulae in Pearson's discussion of Galton's Difference Problem which are not unlike parts of the formulae in Appendix II. On reading Galton's article I feel that there really is something in common between the nature of the Difference Problem and that of the Method of Serial Groups.

[3] *Rep. Cambridge Anthrop. Expedition to Torres Straits*, Cambridge, 1903, II. 190—193.

[4] *Psychol. Rev.* 1902, IX. 444—447.

APPENDIX II

GENERAL EQUATIONS OF THE METHOD OF SERIAL GROUPS.

Let there be n stimulus values

$$r_1 < r_2 < r_3 < \dots < r_n.$$

Let the probability of an answer *two* at each as ascertained by experiment be

$$p_1 < p_2 < p_3 < \dots < p_n.$$

Then the probability of an answer not two is at each

$$q_1 > q_2 > q_3 > \dots > q_n,$$

where $$p_\kappa + q_\kappa = 1.$$

Consider descending series of groups, let there be g double touches in each group, and let that group in each descending series in which the number of correct answers first falls to t or below t be called the critical group. Then the probability that the critical group will be observed at r_κ is given by

$$\mathfrak{P}_\kappa' = \prod_{\lambda=n}^{\lambda=\kappa+1} l_\lambda - \prod_{\lambda=n}^{\lambda=\kappa} l_\lambda \quad \dots\dots\dots\dots\dots\dots\text{(I)},$$

where l_λ = sum of the first $g-t$ terms of

$$(p_\lambda + q_\lambda)^g.$$

The arithmetic mean of a large number of observations of the critical point is then given by

$$\mathfrak{C}' = \sum_{\kappa=1}^{\kappa=n} \mathfrak{P}_\kappa' r_\kappa \quad \dots\dots\dots\dots\dots\dots\text{(II)}.$$

The probable error $\mathfrak{F}'$ of $\mathfrak{C}'$ depends ultimately on the probable errors of the p's. We have

$$\omega_\kappa = \text{probable error of } p_\kappa = \rho\sqrt{\frac{2p_\kappa q_\kappa}{s_\kappa}},$$

where $s_\kappa + 1$ = number of double touches at r_κ.

If N = total number of double touches then the most probable value for $s_\kappa + 1$ is

$$s_\kappa + 1 = \frac{\sum\limits_{\lambda=1}^{\lambda=\kappa} \mathfrak{P}_\lambda'}{\sum\limits_{d=1}^{d=n} \sum\limits_{\lambda=1}^{\lambda=d} \mathfrak{P}_\lambda'}.$$

The probable error $\mathfrak{W}_\kappa'$ of $\mathfrak{P}_\kappa'$ is then given by

$$\begin{aligned}\mathfrak{W}_\kappa'^2 &= \sum_{\lambda=n}^{\lambda=\kappa}\left(\frac{\partial\mathfrak{P}_\kappa'}{\partial p_\lambda}\right)^2 \omega_\lambda^2 \\ &= 2\rho^2\left[\frac{\underline{|g}}{\underline{|t}\,\underline{|g-t-1}}\right]^2 \mathfrak{P}_\kappa'^2\left\{\sum_{\lambda=n}^{\lambda=\kappa+1}\frac{p_\lambda^{2t+1} q_\lambda^{2g-2t-1}}{s_\lambda l_\lambda^2} + \frac{p_\kappa^{2t+1} q_\kappa^{2g-2t-1}}{s_\kappa (1-l_\kappa)^2}\right\} \\ &= 2\rho^2 C'^2 \mathfrak{P}_\kappa'^2 \mathfrak{S}_\kappa' \quad \dots\dots\dots\dots\dots\dots\text{(III)},\end{aligned}$$

where C and $\mathfrak{D}'$ are substituted for the complex expressions: and the probable error $\mathfrak{F}'$ of $\mathfrak{T}'$ is given by

$$\mathfrak{F}'^2=\sum_{\kappa=1}^{\kappa=n}\mathfrak{M}_\kappa'^2 r_\kappa^2$$

$$=2\rho^2C^2\sum_{\kappa=1}^{\kappa=n}\mathfrak{P}_\kappa'^2\,\mathfrak{D}_\kappa' r_\kappa^2 \quad\ldots\ldots\ldots\ldots\ldots\ldots(\text{IV}).$$

APPENDIX III

McDOUGALL'S DEVICE OF DOUBLING THE CRITICAL GROUP.

This device is applicable whatever values be chosen for t and g. McDougall himself used groups of five and descended until a wrong answer was obtained, but this was not recorded as the critical point unless a repetition of this group again gave a wrong answer. The threshold thus measured is clearly lower than the simple four-fifths threshold. He does not say definitely what he would do if the first group gave more than one wrong answer, but if we assume that in this case also the observation is not recorded unless a repetition of the group also gives a wrong answer we get the following method:

(*a*) At no stimulus value above the critical value has the proportion four-fifths or less occurred twice in two trials.

(*b*) At the critical point the proportion four-fifths or less occurs twice running.

We then have equations (I) and (II) as before, but now

$$l_\lambda=1-(1-p_\lambda^5)^2,$$

and in equation (III)

$$C=10,$$

$$\mathfrak{D}_\kappa'=\sum_{\lambda=n}^{\lambda=\kappa+1}\frac{(1-p_\lambda^5)^2 q_\lambda}{s_\lambda p_\lambda(2-p_\lambda^5)^2}+\frac{p_\kappa^9 q_\kappa}{s_\kappa(1-p_\kappa^5)^2}.$$

Equation (IV) then gives $\mathfrak{F}'$.

APPENDIX IV

CRITERION OF COMPARATIVE ACCURACY.

If a threshold $\mathfrak{T}_1$ is measured in N_1 observations and has a probable error $\mathfrak{F}_1$, while another threshold $\mathfrak{T}_2$ with N_2 observations has a probable error $\mathfrak{F}_2$, then in order to compare the accuracy of the two methods we must enquire what probable error $\mathfrak{T}_1$ would have shown had N_2 observations been made in determining it, and we find it would have had a probable error

$$\mathfrak{F}_1\sqrt{\frac{N_1}{N_2}}.$$

Further, it is not the absolute size of the probable error but the proportion which it bears to the threshold which is important. We must, therefore, compare

$$\frac{\mathfrak{F}_1}{\mathfrak{T}_1}\sqrt{\frac{N_1}{N_2}} \text{ with } \frac{\mathfrak{F}_2}{\mathfrak{T}_2}.$$

The first method will be more accurate than the second if

$$\frac{\mathfrak{C}_1}{\mathfrak{F}_1\sqrt{N_1}}>\frac{\mathfrak{C}_2}{\mathfrak{F}_2\sqrt{N_2}}.$$

The number N for each method except Mc Dougall's will be equal to

$$(s_n+1)\sum_{\kappa=1}^{\kappa=n}\sum_{\lambda=1}^{\lambda=\kappa}\mathfrak{P}_\lambda',$$

where s_n+1 is the (large) total number of double touches at r_n where all the descents start, in the case in question fifty at 5 cms. In Mc Dougall's method this expression must be increased by

$$(s_n+1)\sum_{\kappa=1}^{\kappa=n}(1-p_\kappa{}^\delta)\sum_{\lambda=1}^{\lambda=\kappa}\mathfrak{P}_\lambda'$$

because of the doubling when a critical group is suspected.

If we put as an abbreviation

$$A=\frac{\mathfrak{C}'}{\mathfrak{F}'\sqrt{N}}$$

then the equation system

$$\frac{dA}{dn}=\frac{dA}{dg}=\frac{dA}{dt}=0\text{..................................}(\text{V}),$$

gives a relation between n, g and t on the one hand, and the p's and r's on the other, which must be fulfilled for maximum accuracy. The second derivatives must be negative.

APPENDIX V

CALCULATION OF THE 50 °/₀ POINT FROM 80 °/₀ POINT DATA.

This can be done in two ways. If the original data are available *in extenso* the p's will be known for each r in a certain range. If only descents were used the lower p's will be very inaccurate, less so, however, if Mc Dougall's doubling principle was used, for it increases the experiments at lower values of p. The upper p's, however, down to about the middle of the data, will be fairly good, to them any Constant Process can be applied assuming any psychometric function, and by extrapolation the 0·5 point found. If the original data only recorded the critical groups, or if for any other reason the p's are unknown, they can be obtained from the distribution of the critical groups by using equation I.

For example, the data given in Table VI (this *Journal*, v. 215) show that for that subject the series of values of p was

$r=$	0	0·5	1	1·5	2	2·5	3	3·5	4	4·5	5 cms.
$p=$	·028	·06	·12	·24	·48	·64	·82	·90	·96	·98	·96

If in that Table we suppose each descent to stop at the critical group we get

$p=$	—	—	—	—	·40	·90	·90	·90	·95	·98	·96

The top four of these are fairly good values. If we assume only the critical groups to be known, apply equation I and work from 5 cms. downwards we get

$p=$	—	—	—	—	—	·78	·80	·90	·93	·98	·98

Obviously extrapolation from either of these two series to find $p=·5$ is of little value.

CORRELATIONS OF SUMS OR DIFFERENCES.

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AFTER calculating the correlations between several series of values, it frequently happens that we want the correlations given by some of the series added together; or, what comes to the same thing, we want the correlations of the average of some of the series.

Suppose, for instance, that we have tested a number of children on two separate occasions. We are bound to work out the two sets of results separately, in order to ascertain how far they differ from one another, and whether the differences indicate a change of experimental conditions or may reasonably be ascribed to mere 'chance.' But having done this, we next generally desire the result of taking the mean of both occasions. Or again, suppose that we have measured the correlations of the accuracy and also of the speed of any performance; it is almost always interesting to regard the performance as a whole, allotting marks partly for speed and partly for accuracy. Or once more, supposing that we have found out the correlations between a number of experimental tests and position in school, we may wish to learn how far the school position correlates with all the tests pooled together.

Scarcely less important than the correlations between sums are those between differences. When, for instance, children have been tested twice, our chief interest might be in their improvement; we wish to get the correlations of this improvement, that is, of the remainder obtained by subtracting the first result from the second.

But the calculations involved in obtaining the correlations for the sums or the differences are generally laborious. Before we can start working out the further coefficients required, we have to make the necessary additions or subtractions of the original values. And before commencing even this operation, we are obliged to reduce these values to suitable proportions to one another; if, as is usually the case, we desire all the series compounded to have an equal influence on any ensuing correlation, we must multiply them all by such factors as will make their 'standard deviations[1]' equal.

There are many problems, too, where our long work would not even be successful in the end. Suppose, in the instance above, that we were not content to correlate the school position with a pool in which all the tests stood on an equal footing, but aspired to find the proportions in which these tests should be combined in order to make the correlation with school work as large as possible. Such a problem would rarely admit of satisfactory direct attack, however much labour we were willing to expend.

The desired calculation may be not only unfeasible in ordinary practice but essentially impossible. In fact, one may say that this almost invariably happens; for nearly every value attainable by us is more or less vitiated by being only a representative *sample* of the entire class with which we really wish to deal. We should like to obtain and pool such values in infinite number.

Take, for instance, all correlations between mental performances; the power exhibited by a person on any occasion, whether in discriminating pitch or in memorising poetry, in discursive reasoning or in aesthetic combination, is no more than a sample of his general power for the kind of act in question; another sample might well yield a different result. Or the required correlation may be between series of frequencies; for instance, we may want to know how far the tendency in children to be 'naughty' in bad weather is a sign of neurotic disposition; the frequencies actually observed are necessarily infinitely few as compared with the whole class under consideration. The same may be said of correlations between series of correlations. In all cases alike, the desired results of universal validity would have to be based on an infinite number of samples or cases. Actually to obtain and pool these is, of course, out of the question; but at any rate, it is conceivably possible and eminently desirable to calculate the most probable value of such a pool.

The aim of this paper, then, is to save labour and to overcome obstacles to research by expressing the required correlation between sums (or differences) as a simple function of the correlations between the elements combined into these sums (or differences). Let the two groups of elements to be combined be denoted by $a_1, a_2, \ldots a_p$ and

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[1] Using this expression as a short equivalent for 'root mean square deviation.'

$b_1, b_2, \ldots b_q$. In the usual case, where these elements are to be reduced to equal standard deviations before combination, it may be shown that

$$r_{(a_1+a_2+\ldots+a_p)\,(b_1+b_2+\ldots+b_q)} = \frac{\sqrt{pq}\,\bar{r}_{ab}}{\sqrt{1+(p-1)\bar{r}_{aa}}\,\sqrt{1+(q-1)\bar{r}_{bb}}} \ldots\ldots(1)$$[1],

where the term on the left is the required correlation between the sum of the a's and the sum of the b's; $\bar{r}_{aa}$ is the mean correlation between all different pairs of a's; $\bar{r}_{bb}$ is that between all different pairs of b's; and $\bar{r}_{ab}$ is that between all different pairs of an a and a b.

This equation may sometimes more conveniently be written in the form

$$r_{(a_1+a_2+\ldots+a_p)\,(b_1+b_2+\ldots+b_q)} = \frac{S(r_{ab})}{\sqrt{p+2S(r_{aa})}\,\sqrt{q+2S(r_{bb})}} \ldots\ldots(2)$$[1],

where $S(r_{aa})$ is the sum of all correlations between different pairs of a's; $S(r_{bb})$ is that between all different pairs of b's; and $S(r_{ab})$ is that between all different pairs of an a and a b.

From (2) we can readily get the case where the standard deviations of the a's or of the b's are unequal. We have only to multiply every r contained in equation (2) by the standard deviations of the two series entering into this r. The whole equation then becomes

$$r_{(a_1+\ldots+a_p)\,(b_1+\ldots+b_q)} = \frac{S(\sigma_a\sigma_b r_{ab})}{\sqrt{S(\sigma_a^2)+2S(\sigma_a\sigma_a r_{aa})}\,\sqrt{S(\sigma_b^2)+2S(\sigma_b\sigma_b r_{bb})}} \ldots\ldots(3)$$[1],

where $S(\sigma_a^2)$ is the sum of the squared standard deviations of the a's; $S(\sigma_b^2)$ is that of the b's; $S(\sigma_a\sigma_a r_{aa})$ is the sum of the correlations between all different pairs of a's, each such correlation being multiplied by the σ's of the two a's concerned; and $S(\sigma_b\sigma_b r_{bb})$ and $S(\sigma_a\sigma_b r_{ab})$ are analogous values.

Sometimes it may be desired to multiply some or all of the variables $a_1, a_2, \ldots a_p, b_1, b_2, \ldots b_q$ by any constants $n_1, n_2, \ldots n_p, m_1, m_2, \ldots m_q$. To get this, we have only to multiply every σ in equation (3) by the n (or m) belonging to the same a (or b). Whether these constants be positive or negative integers or fractions, we obtain

$$r_{(n_1a_1+\ldots+n_pa_p)\,(m_1b_1+\ldots+m_qb_q)} = \frac{S(nm\sigma_a\sigma_b r_{ab})}{\sqrt{S(n^2\sigma_a^2)+2S(nn\sigma_a\sigma_a r_{aa})}\,\sqrt{S(m^2\sigma_b^2)+2S(mm\sigma_b\sigma_b r_{bb})}} \ldots(4)$$[1],

where the meaning of the terms is evident from the above.

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[1] Appendix, § 1. An illustrative example is worked out in § 2 of the Appendix.

There is no difficulty now in determining the proportions in which the elements have to be combined, in order to make the correlation of the sum (or difference) as large or as small as possible. Take the simple case of any three variables a_1, a_2 and b. Suppose that it is required to determine n so as to make $r_{(na_1+a_2)(b)}$ a maximum or a minimum, n being any positive or negative integer or fraction. This is done by evaluating the r in accordance with the above (4), and then differentiating[1]. We find that the equation

$$n = \frac{\sigma_{a_2}}{\sigma_{a_1}} \cdot \frac{r_{a_1b} - r_{a_2b} \cdot r_{a_1a_2}}{r_{a_2b} - r_{a_1b} \cdot r_{a_1a_2}} \quad \text{.....................}(5)^2$$

makes $r_{(na_1+a_2)(b)}$ a maximum when r_{a_2b} is greater than $r_{a_1b} \cdot r_{a_1a_2}$, and a minimum when r_{a_2b} is less than $r_{a_1b} \cdot r_{a_1a_2}$. In the former case the minimum, and in the latter case the maximum, is given by

$$n = \pm \infty \quad \text{..............................}(6),$$

where the sign to precede ∞ is the opposite to that of r_{a_1b} for the minimum, and the same as that of r_{a_1b} for the maximum.

By putting any of these values of n into any of the above equations (1) to (4), we get the corresponding values of $r_{(na_1+a_2)(b)}$. The latter becomes simply r_{a_1b} when $n = +\infty$, and $-r_{a_1b}$ when $n = -\infty$. When n takes the value shown in (5), then

$$r_{(na_1+a_2)(b)} = \pm \sqrt{\frac{r^2_{a_1b} r^2_{a_2b} - 2r_{a_1b} \cdot r_{a_2b} \cdot r_{a_1a_2}}{1 - r^2_{a_1a_2}}} \quad \text{.........}(7)^3,$$

taking the plus sign when r_{a_2b} is greater than $r_{a_1b} \cdot r_{a_1a_2}$, and vice versa.

Turn, lastly, to the case where the number of series to be added together is infinite. In actual experience, of course, only a comparatively small number can possibly be given; we can only observe the correlation of a with the sum (or average) of q b's where q is finite. But we may well wish to know the correlation of a with the sum or average of an infinite number of b's that are *similar*, in the sense of having the same average correlation with a and also with one another. From (1) we easily get

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[1] The required 'relative' maximum is obtained by observing the variations of the sign of the first differential (see Serret, *Cour de Calcul différential*, § 146). In many cases it coincides with an 'absolute' maximum, and is therefore among the solutions got from putting the differential = zero.

[2] Here I find myself, as often before, partly anticipated by the correlational researches of Udny Yule. He showed some time ago (as was kindly pointed out to me by Mr Webb) that equation (5) gives the maximum value of $r_{(na_1+a_2)(b)}$ if we disregard sign, which we usually can do (*J. Roy. Statist. Soc.*, 1906, 199). Moreover, even the more general case of the maximum value of $r_{(n_1a_1+\dots+n_pa_p)(b)}$ appears readily soluble by the theory of partial correlations of the same investigator (see his *Introduction to the Theory of Statistics*, 1912, chap. XII. section 16). By the aid of the present theorems, the still more general case of the maximum of $r_{(n_1a_1+\dots+n_pa_p)(m_1b_1+\dots+m_qb_q)}$ can be obtained, though sometimes through rather complicated differentiation.

[3] This root expression is termed by Yule '*R*' and has been shown by him to possess considerable statistical importance (see his *Introduction to the Theory of Statistics*, 243–4).

$$r_{(a)\,(b_1+b_2+\ldots\text{ to infinity})} = r_{(a)\,(b_1+\ldots+b_q)} \frac{\sqrt{1+(q-1)\,\bar{r}_{bb}}}{\sqrt{q\,.\,\bar{r}_{bb}}} \quad \ldots\ldots(8)\text{[1]},$$

and in the same way

$$r_{(a_1+a_2\ldots\text{ to inf.})\,(b_1+b_2\ldots\text{ to inf.})}$$
$$= r_{(a_1+a_2+\ldots+a_p)\,(b_1+b_2+\ldots+b_q)} \frac{\sqrt{1+(p-1)\,\bar{r}_{aa}}\,\sqrt{1+(q-1)\,\bar{r}_{bb}}}{\sqrt{p\,.\,\bar{r}_{aa}}\,\sqrt{q\,.\,\bar{r}_{bb}}} \quad \ldots(8a)\text{[1]},$$

where $\bar{r}_{aa}$ denotes, as before, the average correlation of the p a's with one another, and $\bar{r}_{bb}$ is analogous. We can give to p or to q any values we please, but in practice they are most frequently taken as $=2$.

If we replace $r_{(a_1+\ldots+a_p)\,(b_1+\ldots+b_q)}$ in ($8a$) by its value as given in (1), we get the simple result

$$r_{(a_1+\ldots\text{ to inf.})\,(b_1+\ldots\text{ to inf.})} = \frac{\sqrt{\bar{r}_{ab}}}{\sqrt{\bar{r}_{aa}\,.\,\bar{r}_{bb}}} \quad \ldots\ldots\ldots\ldots\ldots(9)\text{[1]},$$

where $\bar{r}_{ab}$ means, as before, the average correlation between the p a's and the q b's, and the remaining terms have the same significance as in ($8a$).

The above three formulae are *exact* for any values whatever of the a's and the b's, so long as the above 'similarity' holds good, that is, the average correlation between the a's, between the b's, and between the a's and b's is the same on the left side of the equation as on the right. The whole real difficulty lies in the question as to how far our observed series can be expected to give 'similar' correlations to those of the infinite series that we may have in mind.

To begin with, this expectation of similarity is obviously precarious in proportion to the size of the 'probable error' of $\frac{\sqrt{\bar{r}_{ab}}}{\sqrt{\bar{r}_{aa}\bar{r}_{bb}}}$. If $\bar{r}_{aa}$ and $\bar{r}_{bb}$ are very small, this 'probable error' is usually very large, so that the determination of $r_{(a_1+\ldots\text{ to inf.})\,(b_1+\ldots\text{ to inf.})}$ becomes illusory. In practice, I have found it very desirable that $\bar{r}_{aa}$ and $\bar{r}_{bb}$ should amount to ·70 to ·80, and indispensable that they should be at least ·40 to ·50.

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[1] Appendix, § 4.

Further, only in certain cases does the basal conception, that of an infinite number of series giving 'similar' correlations to the series actually observed, possess a useful scientific significance. One such case is where $a_1, a_2, \ldots$ are several measurements of one and the same variable, and differ from one another merely owing to random errors; similarly, as regards $b_1, b_2, \ldots$. The formula for this case has been given in former papers, and will be found to be perfectly corroborated by the present equations (8 *a*) and (9)[1].

Another important case is where the mean of an infinite number of values denotes, not any individual value, but some *collective characteristic* (as average, dispersion, etc.) of the ideal frequency distribution of infinitely numerous results, for which certain conditions have remained constant while other influences have varied in a random manner. Under this heading would come all tests of mental powers, as discussed above.

Or again, the mean of infinitely numerous values is often of use even when it represents some magnitude plus a definite error. The measurements of anything will be liable to disturbances which are in part random but in part constantly biased in some particular direction. On taking the mean of an infinite number of such measurements, the random errors will be completely eliminated; the constant error will persist, indeed, but now it will be cleared of complication and amenable to special treatment[2]. This applies equally to collective characteristics; for these, too, are liable to distortion by constant influences—whether downright errors, or merely tendencies irrelevant to the point at issue—which must be treated just like the constant errors that occur in measuring an individual.

The chief difficulty arises when, in addition or not to any random and any constant errors, there are slow systematic changes, like those produced in mental performances by exercise and fatigue. Here, the application and interpretation of (8*a*) and (9) will need care[3]. But nearly always, the values obtained will be the sums or averages of several single observations; for instance, a performance of memory will not be measured by the correctness of one or even two answers only, but by that of a considerable number. And these single observations can easily be arranged in two groups, a_1 and a_2, whose difference with respect to the slow changes is negligible[1]. Hereupon, (8a) and (9) become applicable with the same signification as in the two preceding paragraphs.

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[1] This *Journal*, 1910, III. p. 275; *Amer. Journ. of Psychol.* 1904, XV. p. 290. Some critics have wrongly said that the original formula was subsequently changed. All that has since been done is to render it much more general and to improve its application.

[2] See this *Journal*, 1910, III. 279.

[3] Under such circumstances, (8 *a*) and (9) will sometimes yield values which are impossible, not lying between +1 and −1. This does not show that the equation is wrong, but only that the case of which they treat—an infinite number of *a*'s and *b*'s presenting on an average the same sized inter-correlations as the *a*'s and *b*'s observed—cannot occur with the given values (usually, owing merely to their errors of sampling). This impossibility can be traced to the logical requirement that, whatever series 1, 2, and 3 may denote, r_{23} must necessarily lie between the limits: $r_{12}r_{13} \pm \sqrt{1 - r_{12}^2 - r_{13}^2 - r_{12}^2 r_{13}^2}$ (see Yule, *ibid.* p. 246). The converse fact is worth remembering: whenever (8 *a*) and (9)

Further uses of all the equations given above will readily suggest themselves to any one familiar with correlational work. For example, (1) furnishes at once "the increase of correlation between two different characters, to be obtained by increasing the number of measurements," for which the writer has given a formula in a previous paper[2]; in fact, all the main formulae more or less elaborately demonstrated in that paper turn out to be immediate corollaries of our simply obtained (1)[3].

Many results of other writers can be got with equal facility. For instance, a few months ago Woodworth proved that the correlation of $(x+y)$ with x is $1/\sqrt{2}$[4]. But if in (1) we put $a_1 = b = x$ and $a_2 = y$, we have immediately

$$r_{(x+y)(x)} = \frac{\sqrt{2}\times\frac{1}{2}}{\sqrt{1+0}\,\sqrt{1}} = 1/\sqrt{2}.$$

Further instances have appeared above in the values that had been previously reached in various manners by Udny Yule.

To sum up, the cases where we need the correlations of sums, averages, and differences are numerous and important. The above simple formulae both aid in obtaining them, and also show that the customary replacement of the correlation of averages by the average of correlations [namely, $r_{(a_1+\ldots+a_p)(b_1+\ldots+b_q)}$ by $\bar{r}_{ab}$] cannot under any circumstances lead to the right result. It should also be noticed that all the above formulae are in themselves not approximate but exact; any inaccuracy in the conclusions got through their use can only mean

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give values between +1 and −1, then it is possible for an infinite number of a's and b's to give on an average the same sized inter-correlations as those already observed.

[1] Usually it will be sufficient to pool the odd single values for a_1, and the even ones for a_2.

[2] This *Journal*, 1910, III. 281.

[3] Hence it was a mistake in that paper to suppose them at all independent of one another (see top of p. 282).

[4] *Psychol. Rev.* 1912, XIX. 113. He assumed, as we do here, that the standard deviations of x and y are equal.

that the terms in the formulae are untruly represented by the empirical data employed or by the theoretical interpretations given. Also, the formulae do not involve either 'normality' in the distribution of the variables or 'linearity' in the correlations[1]; they make no assumptions beyond those of logic.

APPENDIX.

§ 1. Let the two series of variables to be summed be denoted by $a_1, a_2, \ldots a_p$ and $b_1, b_2, \ldots b_q$, each being measured from its own mean and consisting of N cases. Let these variables be multiplied respectively by the constants $n_1, n_2, \ldots n_p$ and $m_1, m_2, \ldots m_q$. Let the required correlational coefficient between

$$n_1a_1+n_2a_2+\ldots+n_pa_p \text{ and } m_1b_1+m_2b_2+\ldots+m_qb_q$$

be denoted by $r_{(n_1a_1+\ldots+n_pa_p)(m_1b_1+\ldots+m_qb_q)}$.

Then the numerator of this coefficient will evidently

$$=\sum_1^N\left[\overset{p}{\underset{1}{S}}(na)-\frac{1}{N}\sum_1^N\overset{p}{\underset{1}{S}}(na)\right].\sum_1^N\left[\overset{q}{\underset{1}{S}}(mb)-\frac{1}{N}\sum_1^N\overset{q}{\underset{1}{S}}(mb)\right],$$

where S denotes summation of the p or q different variables, and Σ denotes summation of the N different cases.

This expression reduces to

$$NS_{st}(n_sm_t\sigma_{a_s}\sigma_{b_t}r_{a_sb_t}),$$

where $r_{a_sb_t}$ is the correlation between a_s and b_t, the two σ's are the standard deviations of these two variables, and s and t take all values from 1 up to p and q respectively. Putting similar terms in the denominator, we get the required coefficient,

$$r_{(n_1a_1+\ldots+n_pa_p)(m_1b_1+\ldots+m_qb_q)}=\frac{S_{st}(n_sm_t\sigma_{a_s}\sigma_{b_t}r_{a_sb_t})}{\sqrt{S^*_{st}(n_sn_t\sigma_{a_s}\sigma_{a_t}r_{a_sa_t})}\sqrt{S^*_{st}(m_sm_t\sigma_{b_s}\sigma_{b_t}r_{b_sb_t})}} \ldots(10),$$

the stars indicating that these sums include the cases where a_s and b_s denote the same variables as a_t and b_t respectively,

$$=\frac{S_{st}(n_sm_t\sigma_{a_s}\sigma_{b_t}r_{a_sb_t})}{\sqrt{S_s(n_s^2\sigma_{a_s}^2)+2S_{st}(n_sn_t\sigma_{a_s}\sigma_{a_t}r_{a_sa_t})}\sqrt{S_s(m_s^2\sigma_{b_s}^2)+2S_{st}(m_sm_t\sigma_{b_s}\sigma_{b_t}r_{b_sb_t})}} \ldots(11),$$

where a_s and b_s always denote different values from a_t and b_t respectively.

When each n and m is equal to unity, and the σ's of the a's are equal to one another, and similarly the σ's of the b's, then on reduction

$$r_{(a_1+\ldots+a_p)(b_1+\ldots+b_q)}=\frac{S_{st}(r_{a_sb_t})}{\sqrt{p+2S_{st}(r_{a_sa_t})}\sqrt{q+2S_{st}(r_{b_sb_t})}} \ldots\ldots\ldots\ldots(12),$$

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[1] Although non-linearity cannot affect the relations given in this paper between the r's, it can, of course, influence the significance of these r's. But to say that extreme non-linearity makes r meaningless overlooks the distinction between correlation and dependence (Yule, *ibid.* p. 174) and the extraordinarily wide-reaching statistical properties of r in any case.

which may, sometimes more conveniently, be written as

$$r_{(a_1+\ldots+a_p)(b_1+\ldots+b_q)}=\frac{\sqrt{pq}\,.\,\bar{r}_{ab}}{\sqrt{1+(p-1)\,\bar{r}_{aa}}\,\sqrt{1+(q-1)\,\bar{r}_{bb}}}\ \ldots\ldots\ldots\ldots\ldots(13),$$

where $\bar{r}_{ab}$ denotes the mean correlation between the a's and b's, and $\bar{r}_{aa}$ and $\bar{r}_{bb}$ are analogous values.

§ 2. An illustrative example of the above.

Suppose that we want the standard deviation of a_2 to be double that of a_1, and that of b_2 to be treble that of b_1, while, further, b_2 is to be multiplied by $-\cdot 3$. Then, by (11), the required coefficient

$$r_{(a_1+a_2)(b_1-\cdot 3b_2)}=\frac{\sigma_{a_1}\sigma_{b_1}r_{a_1b_1}-\cdot 3\sigma_{a_1}\sigma_{b_2}r_{a_1b_2}+\sigma_{a_1}\sigma_{b_1}r_{a_2b_1}-\cdot 3\sigma_{a_2}\sigma_{b_2}r_{a_2b_2}}{\sqrt{\sigma_{a_1}{}^2+\sigma_{a_2}{}^2+2\sigma_{a_1}\sigma_{a_2}r_{a_1a_2}}\sqrt{\sigma_{b_1}{}^2+\cdot 09\sigma_{b_2}{}^2-\cdot 6\sigma_{b_1}\sigma_{b_2}r_{b_1b_2}}},$$

and, dividing out by $\sigma_{a_1}\sigma_{b_1}$,

$$=\frac{r_{a_1b_1}-\cdot 3r_{a_1b_2}+r_{a_2b_1}-\cdot 3r_{a_2b_2}}{\sqrt{5+4r_{a_1a_2}}\sqrt{1\cdot 81-1\cdot 8r_{b_1b_2}}}.$$

§ 3. To find the maximum and minimum of $r_{(na_1+a_2)(b)}$ for varying n.

By (11) the above coefficient

$$=\frac{n\sigma_{a_1}\sigma_b r_{a_1b}+\sigma_{a_2}\sigma_b r_{a_2b}}{\sqrt{n^2\sigma_{a_1}{}^2+\sigma_{a_2}{}^2+2n\sigma_{a_1}\sigma_{a_2}r_{a_1a_2}}\sqrt{\sigma_b{}^2}},$$

so that its first differential, on some reduction,

$$=\frac{n\sigma_{a_1}{}^2\sigma_{a_2}(r_{a_1a_2}r_{a_1b}-r_{a_2b})+\sigma_{a_1}\sigma_{a_2}{}^2(r_{a_1b}-r_{a_1a_2}r_{a_2b})}{(n^2\sigma_{a_1}{}^2+\sigma_{a_2}{}^2+2n\sigma_{a_1}\sigma_{a_2}r_{a_1a_2})^{\frac{3}{2}}}\ldots\ldots\ldots\ldots\ldots(14).$$

Now, take first the case that $r_{a_2b}>r_{a_1a_2}r_{a_1b}$.

Let n vary continuously from $-\infty$ to $+\infty$. At the start, the above differential $=\frac{r_{a_2b}-r_{a_1b}r_{a_1a_2}}{\infty^2}$, which is positive; its denominator being so always, the differential remains positive until its numerator passes through zero, whereupon it becomes and remains negative. Hence, the required maximum occurs when its numerator $=0$, namely, when

$$n=\frac{\sigma_{a_2}}{\sigma_{a_1}}\frac{r_{a_1b}-r_{a_2b}r_{a_1a_2}}{r_{a_2b}-r_{a_1b}r_{a_1a_2}}\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(15).$$

When $n=-\infty$ and $=+\infty$, $r_{(na_1+a_2)(b)}$ reduces to $-r_{a_1b}$ and to $+r_{a_1b}$ respectively; clearly, whichever of these two values is negative is the required minimum.

Take, next, the case that $r_{a_2b}<r_{a_1b}r_{a_1a_2}$.

By similar reasoning to the above, $r_{(na_1+a_2)(b)}$ can be shown to attain its maximum at the positive one of the two values, r_{a_1b} and $-r_{a_1b}$; also to attain its minimum when n becomes the value given in (15).

§ 4. To find the value of $r_{(a_1+a_2+\dots\text{to infinity})(b_1+b_2+\dots\text{to infinity})}$.

The required coefficient, using (13),

$$=\frac{\sqrt{\infty\times\infty}\,\bar{\bar{r}}_{ab}}{\sqrt{1+(\infty-1)\bar{\bar{r}}_{aa}}\sqrt{1+(\infty-1)\bar{\bar{r}}_{bb}}}=\frac{\bar{\bar{r}}_{ab}}{\sqrt{\bar{\bar{r}}_{aa}\,.\,\bar{\bar{r}}_{bb}}},$$

where $\bar{\bar{r}}_{ab}$ indicates the mean correlation of the infinitely numerous *a*'s with the infinitely numerous *b*'s, and $\bar{\bar{r}}_{aa}$ and $\bar{\bar{r}}_{bb}$ have analogous meanings.

This becomes, on multiplying it by the left-hand term and dividing by the right-hand term of (13),

$$r_{(a_1+a_2+\dots+a_p)(b_1+b_2+\dots+b_q)}\cdot\frac{\sqrt{1+(p-1)\bar{r}_{aa}}\sqrt{1+(q-1)\bar{r}_{bb}}}{\sqrt{pq}\,.\,\bar{r}_{ab}}\cdot\frac{\bar{\bar{r}}_{ab}}{\sqrt{\bar{\bar{r}}_{aa}\,.\,\bar{\bar{r}}_{bb}}},$$

where p and q have any values we please; and, if $\bar{r}_{aa}=\bar{\bar{r}}_{aa}$, $\bar{r}_{bb}=\bar{\bar{r}}_{bb}$, $\bar{r}_{ab}=\bar{\bar{r}}_{ab}$, the required coefficient

$$=r_{(a_1+a_2+\dots+a_p)(b_1+b_2+\dots+b_q)}\frac{\sqrt{1+(p-1)\bar{r}_{aa}}}{\sqrt{p\bar{r}_{aa}}}\cdot\frac{\sqrt{1+(q-1)\bar{r}_{bb}}}{\sqrt{q\bar{r}_{bb}}}\quad\dots\dots\dots(16).$$

(*Manuscript received* 5 *January* 1913.)

A STUDY OF MENTAL FATIGUE[1].

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I. The method of producing mental fatigue.

II. Analysis of the experimental results.

  (i) *Fatigue as manifested by decrease in speed.*

  (ii) *Fatigue as manifested by decrease in accuracy.*

III. Description of the tests for fatigue applied before and after the principal work.

IV. Analysis of the records of the tests, and the evidence of significant correlation between changes in the accuracy and output of mental work on the one hand and the response of the subjects to the test on the other.

V. General conclusions.

VI. Appendix.

I. THE METHOD OF PRODUCING MENTAL FATIGUE.

ALL the experiments were carried out at the Physiological Laboratory of the London University. They were arranged in three series; Series I took place during July 1907; Series II during the latter end of September and beginning of October; a further investigation dealing with the same questions was undertaken in the following January, which will be referred to as Series III.

The mental work selected for these experiments consisted in the multiplication of three figures 'in the head,' the product of the first two being multiplied by the third. The sums were worked in

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[1] **Part of a thesis approved for the degree of Doctor of Science in the University of London. The writer wishes to acknowledge the help and advice she has received from Miss Edgell and Dr Myers in the course of her work.**

Kräpelin's *Rechenhefte*, and the answers were written out by the side of the column. This was done in order that the actual number of faults in the calculations might be determined, and thereby the quality of the work be estimated.

Series I and II each consisted of five experimental days, and save in date were alike in every particular. The multiplication continued for one hour, and the tests before and after the hour occupied approximately thirty minutes. A pause of ten minutes was allowed on every alternate day, after thirty minutes' work. These days will be spoken of as 'pause days' in contradistinction to the 'non-pause days.' Each series began with a pause day. At the end of every five minutes a bell attached to a clock sounded softly, and the subject marked the place in the column of figures. The general conditions of life were kept as uniform as possible by the three subjects who undertook the experiments. They did their utmost to maintain the highest speed and the greatest accuracy; they felt that the work involved considerable strain. Each kept introspective notes, the evidence of which was helpful afterwards in the explanation of certain variations. As the results of the arithmetic were not analysed until the end of the series, the subjects were uninfluenced by the knowledge of the previous day's progress.

The tests employed before and after the mental work consisted in the determination of:

(i) the spatial threshold,
(ii) the muscular capacity,
(iii) the rate of respiration,
(iv) the rate of the pulse,
(v) the speed and accuracy of perception.

The methods of taking these tests are explained in detail later (pages 434, 435).

Series III, undertaken only by two of the three subjects, consisted of four experimental days, which were each divided into two work periods, one hour in the morning and two consecutive hours in the afternoon. The work and general method of experiment were the same as in Series I and II, but no pause was allowed within either period of work. The tests employed before and after each work period comprised the determination of:

(i) the spatial threshold,
(ii) the muscular capacity.

II. ANALYSIS OF THE EXPERIMENTAL RESULTS.

(i) *Fatigue as manifested by decrease in speed.*

In order to ascertain the decrease in speed, the second half-hour's work is compared with the first. The average number of sums accomplished in the first period has been taken as 100 and that of the second half-hour as a percentage of the first.

TABLE I.

Showing the average number of sums worked in each half-hour.

	Pause days		Non-pause days	
	First ½ hour	Second ½ hour	First ½ hour	Second ½ hour
Series I				
Subject E	100	104·9	100	92·6
„ R	100	98·5	100	91·0
„ M	100	94·4	100	92·7
Series II				
Subject E	100	101·5	100	94·8
„ R	100	98·3	100	106·5
„ M	100	102·6	100	100·4

With the exception of R in Series II, there is a greater tendency for the speed to diminish in the second half-hour of the 'non-pause days' than in the corresponding time of the 'pause days.'

In order to emphasize this diminution in the speed of working, the number of sums worked in each of the last two fifteen-minute periods has been compared with the number in the second fifteen-minute period (Table II).

A comparison between the 'pause' and 'non-pause' days shows that with subject E a diminution in rate immediately after the pause occurs only once. In both series the acceleration (which is probably due to the recovery from fatigue) is more evident, *i.e.* loss of 'incitement' (*Anregung*) is more than compensated for by recuperation.

Subject R appears to have suffered from loss of incitement during the pause, while the recuperative effect was small.

Subject M responded very indefinitely to the pause. Neither the immediate loss of incitement nor secondary recuperation are at all evident; the rate was only slightly diminished and the recovery no better than on 'non-pause' days.

TABLE II.

Showing the ratio of the number of sums in the third and fourth fifteen-minute periods to that of the second expressed as 100.

Subject E

	Series I				*Series II*		
Date	**II**	**III**	**IV**	**Date**	**II**	**III**	**IV**
July 13	**100 pause**	**106·2**	**128·1**	**Sept. 18**	**100 pause**	**110·0**	**105·0**
,, 20	**100 ,,**	**72·7**	**92·0**	**,, 25**	**100 ,,**	**121·3**	**105·8**
,, 27	**100 ,,**	**102·7**	**127·2**	**Oct. 2**	**100 ,,**	**107·1**	**110·2**
July 17	**100 no pause**	**88·8**	**108·2**	**Sept. 21**	**100 no pause**	**85·7**	**84·6**
,, 24	**100 ,,**	**78·1**	**96·1**	**,, 28**	**100 ,,**	**115·7**	**106·1**

Subject R

	Series I				*Series II*		
Date	**II**	**III**	**IV**	**Date**	**II**	**III**	**IV**
July 13	**100 pause**	**107·8**	**103·6**	**Sept. 18**	**100 pause**	**93·8**	**98·6**
,, 20	**100 ,,**	**77·7**	**89·9**	**,, 25**	**100 ,,**	**93·5**	**106·8**
,, 27	**100 ,,**	**99·4**	**112·2**	**Oct. 2**	**100 ,,**	**93·6**	**102·9**
July 17	**100 no pause**	**108·1**	**94·7**	**Sept. 21**	**100 no pause**	**129·0**	**113·9**
,, 24	**100 ,,**	**93·5**	**102·7**	**,, 28**	**100 ,,**	**106·0**	**100·4**

Subject M

	Series I				*Series II*		
Date	**II**	**III**	**IV**	**Date**	**II**	**III**	**IV**
July 13	**100 pause**	**97·3**	**92·9**	**Sept. 18**	**100 pause**	**84·6**	**98·0**
,, 20	**100 ,,**	**75·3**	**90·4**	**,, 25**	**100 ,,**	**78·9**	**120·2**
,, 27	**100 ,,**	**88·9**	**96·9**	**Oct. 2**	**100 ,,**	**99·6**	**104·7**
July 17	**100 no pause**	**82·2**	**106·0**	**Sept. 21**	**100 no pause**	**94·5**	**100·3**
,, 24	**100 ,,**	**99·6**	**95·4**	**,, 28**	**100 ,,**	**96·0**	**111·5**

The introspective notes of the subjects are in agreement with these results. Subject E felt she was working more quickly after the pause. Subject R laid emphasis on the difficulty of re-starting the work after the interruption and did not appreciate the rest. Subject M reported herself as quite indifferent to the effect of the pause; she did not feel it to be either a help or a hindrance.

Examination of the work done on the four non-pause days of Series I and II shows that in the second half-hour the speed diminished three times with subject E, twice with R, and twice with M. In Series III comparison of the last half-hour with the first half-hour of each work period shows that the speed diminished twice with subject R and once with M in Period I, and twice with R and three times with M in Period II (see Table III).

TABLE III.

Showing the number of sums worked in each thirty minutes expressed as the percentage of the first.

Non-pause days

Subject E

Series I	30 mins.	30 mins.	*Series II*	30 mins.	30 mins.
July 17	100	92	Sept. 21	100	86
„ 24	100	93	„ 28	100	104

Subject R

Series I	30 mins.	30 mins.	*Series II*	30 mins.	30 mins.
July 17	100	85	Sept. 21	100	91
„ 24	100	101	„ 28	100	110

Subject M

Series I	30 mins.	30 mins.	*Series II*	30 mins.	30 mins.
July 17	100	88	Sept. 21	100	113
„ 24	100	93	„ 28	100	100·2

Series III

Subject R

	Period I. One hour		Period II. Two hours			
	30 mins.	30 mins.	30 mins.	30 mins.	30 mins.	30 mins.
Jan. 8	100	88	93	93	99	91
„ 10	100	99	115	116	114	115
„ 13	100	106	103	111	110	94
„ 15	100	108	108	108	115	108
average	100	100	105	107	110	102

Subject M

	Period I. One hour		Period II. Two hours			
	30 mins.	30 mins.	30 mins.	30 mins.	30 mins.	30 mins.
Jan. 8	100	95	93	79	82	84
„ 10	100	102	102	122	107	101
„ 13	100	103	101	112	107	105
„ 15	100	119	115	114	114	108
average	100	105	103	107	103	100

In all three series involuntary oscillations in speed occurred throughout, so that periods of comparative rest alternated with those of more strenuous work. For these reasons diminution in rate of working was neither sufficiently constant nor continuous to be regarded as a characteristic result of the early stages of fatigue.

(ii) *Fatigue as manifested by decrease in accuracy.*

Only one subject M showed a decrease in accuracy in the second half-hour of each experiment in Series I and II. In order to see what effect the pause had on the accuracy of the work, the percentage of faults in the second period of fifteen minutes has been taken as 100, and the percentages of the third and fourth periods have been expressed as ratios of this. (Table IV.)

TABLE IV.

Showing the ratio of the percentage of faults in the third and fourth fifteen-minute periods to that of the second expressed as 100.

Subject E

Series I				*Series II*			
Date	II	III	IV	Date	II	III	IV
July 13	100 pause	70	117	Sept. 18	100 pause	90	45
,, 20	100 ,,	59	77	,, 25	100 ,,	90	50
,, 27	100 ,,	15	12	Oct. 2	100 ,,	11	0
July 17	100 no pause	160	340	Sept. 21	100 no pause	380	680
,, 24	100 ,,	514	442	,, 28	100 ,,	65	25

Subject R

Series I				*Series II*			
Date	II	III	IV	Date	II	III	IV
July 13	100 pause	111	79	Sept. 18	100 pause	38	113
,, 20	100 ,,	79	50	,, 25	100 ,,	52	168
,, 27	100 ,,	177	77	Oct. 2	100 ,,	375	450
July 17	100 no pause	188	247	Sept. 21	100 no pause	127	261
,, 24	100 ,,	103	60	,, 28	100 ,,	51	42

Subject M

Series I				*Series II*			
Date	II	III	IV	Date	II	III	IV
July 13	100 pause	157	64	Sept. 18	100 pause	100	133
,, 20	100 ,,	102	104	,, 25	100 ,,	89	126
,, 27	100 ,,	102	86	Oct. 2	100 ,,	221	275
July 17	100 no pause	165	95	Sept. 21	100 no pause	331	313
,, 24	100 ,,	141	112	,, 28	100 ,,	164	112

It is evident that the pause had a beneficial effect on the accuracy of the work of subject E. The percentage of faults after the rest is less than that of a similar period of the 'non-pause days.' As was before noticed this subject suffered little from loss of incitement during the pause. The rest was beneficial both from the point of view of speed and from the point of view of accuracy.

The second subject R, whose speed diminished after the pause, shows little improvement in accuracy. In no respect was the rest beneficial.

The accuracy of the 'pause days' of the third subject M was not appreciably different from that of the 'non-pause days.' Under both conditions a decrease is noticed in the third and fourth periods. As in the case of speed neither loss of incitement nor recuperative effect could be attributed to the pause, so here also its effect seems *nil.* One may surmise that for R and M the pause is not of favourable duration. Lindley[1] and Hylan and Kräpelin[2] have shown that the duration of the period in which the loss of incitement is outweighed by the recovery from fatigue varies with different individuals.

Comparison may be made with the results of Series III. In Table V the percentage of faults has been calculated on the number of sums in thirty minutes' work.

TABLE V.

Showing the percentage of faults in each half-hour's work. Series III.

Subject R

	Period I. One hour		Period II. Two hours			
	30 mins.	30 mins.	30 mins.	30 mins.	30 mins.	30 mins.
Jan. 8	2·1 %	2·5 %	3·3 %	2·3 %	3·5 %	1·0 %
,, 10	1·2 ,,	1·4 ,,	1·3 ,,	2·4 ,,	2·4 ,,	2·4 ,,
,, 13	1·2 ,,	2·7 ,,	1·1 ,,	2·9 ,,	3·5 ,,	3·2 ,,
,, 15	1·3 ,,	1·7 ,,	1·3 ,,	1·3 ,,	1·5 ,,	1·3 ,,
average	1·4 %	2·1 %	1·7 %	2·2 %	2·7 %	2·0 %

Subject M

Jan. 8	3·4 %	3·3 %	2·0 %	3·5 %	1·0 %	3·6 %
,, 10	1·3 ,,	3·6 ,,	·9 ,,	1·5 ,,	1·2 ,,	2·1 ,,
,, 13	1·2 ,,	1·1 ,,	1·0 ,,	1·6 ,,	2·4 ,,	2·0 ,,
,, 15	·9 ,,	2·2 ,,	·9 ,,	1·3 ,,	2·1 ,,	1·6 ,,
average	1·7 %	2·5 %	1·2 %	2·0 %	1·6 %	2·3 %

It will be noticed that there is improvement in accuracy in the final thirty minutes of R's work. With M a decrease always occurs though not in proportion to the work done. On two occasions the percentage increase is greater after thirty minutes' than after sixty minutes' work.

It is quite evident from the introspective notes (see Appendix) that the subjects felt distinctly tired at the end of the experiments. The fact that fatigue cannot be demonstrated by diminution of accuracy appears in the case of E to be due to the stability of the habit of accuracy which remained unaffected in the first stages. In the case of R and M, judging from their work and from introspective notes, we find that this habit was not so deeply established, the decrease in accuracy in the case of M being sufficiently constant to be regarded as significant evidence. As the outcome then of the analysis of the work we may conclude that fatigue cannot be invariably estimated by diminution either in speed or in accuracy of work, since habit and method of working bear an important relation to its manifestation.

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[1] *Psychol. Arb.* 1900, III. 502—508.

[2] *Psychol. Arb.* 1902, IV. 473—478.

III. DESCRIPTION OF THE TESTS FOR FATIGUE.

The tests that were selected were the following:

Test I. *The cutaneous spatial threshold.* The average of the threshold for 'twoness' and the threshold for 'oneness' was taken as the threshold for comparison. Contact was made by placing the points of an aesthesiometer horizontally on the left zygomatic arch. Every care was taken that the points touched *simultaneously* and with equal pressure.

The experimenter started with a distance distinctly above that at which the subject first felt the two points as one (this was determined by trial experiments); the distance was diminished at each application at first by 1 mm., and later, on approaching the threshold, by 0·5 mm. If the subject was doubtful, the points were raised and applied again. When the threshold for 'oneness' was reached, the distance was read with the help of a vernier scale in tenths of a millimetre and the results were noted down. The subject was then given a distance below that at which she could first discriminate two points again determined by trial experiments, and the points were separated until the threshold for 'twoness' was reached. The number of contacts with decreasing and with increasing distance varied from ten to five. It was hoped by this method to avoid the criticism made on Griesbach's experiments, that the procedure was too rapid to allow a true judgment to be made.

Test II. *The muscular capacity was ascertained*:

(*a*) by means of a dynamometer.

In Series I and II the subject exerted the utmost pressure with the right hand.

In Series III first with the right hand and afterwards with the left hand.

(*b*) by means of an ergograph in Series III. The subjects used Kräpelin's ergograph, in which the finger is relieved from the strain of holding the weight between each flexion. Hence the total performance, calculated by the weight multiplied by the height to which it is raised, approaches as nearly as possible the actual work done by the muscles.

Test III. *The respiratory movements* were recorded on a smoked drum by means of a pneumograph.

Test IV. The *pulse* was recorded by a Dudgeon's sphygmograph.

Test V. The *accuracy and quickness of perception* were tested in the following way: The subjects were given a pattern drawn on chequered paper. Each pattern consisted of a definite number of squares drawn in two colours. The patterns selected for the two tests of the same day, in spite of difference in arrangement, always contained the same number of squares. The subject was asked to copy the pattern as many times as she could in five minutes; each individual square had to be completed before the next could be drawn. Apart from the act of drawing, the quickness in execution depended on the way in which the grouping of the squares was understood and analysed—an act which demanded voluntary concentration of attention. It was assumed that as the subject became tired, concentration of attention would become more difficult. It was hoped that this failure might be manifested objectively in diminution of the number of squares and in the inaccuracy with which the patterns were reproduced. Each square, correctly placed and finished, counted as 'one.' The method possessed the advantage that owing to the difference in arrangement of the squares the work never became mechanical and was simple to estimate numerically.

The order in which the tests were taken in Series I and II was as follows: (1) muscular capacity, (2) spatial threshold, (3) rate of respiration, (4) rate of pulse, (5) quickness and accuracy of perception. In Series III the following order was observed: (1) muscular capacity tested by dynamometer in right hand, then in left hand, (2) spatial threshold, (3) muscular capacity tested by the ergograph.

IV. ANALYSIS OF THE RECORDS OF THE TESTS.

Fatigue as manifested by alteration of the spatial threshold. In determining the value of this test two points were kept in view: (i) the consistency of the rise in threshold after the multiplication work, (ii) the correlation between this change and those observed in the standard of the hour's work.

With regard to the former point, although it was not assumed that the work would always induce the same condition of fatigue, it was quite certain that on each occasion the subjects were distinctly tired. The readings are given in Table VI.

TABLE VI.

Showing the records of the spatial threshold taken before and after one hour's multiplication.

	Subject E		Subject R		Subject M	
	Before	After	Before	After	Before	After
Series I	mms.	mms.	mms.	mms.	mms.	mms.
July 13	7·5	7·2	7·9	5·6	7	7·5
17	9·75	9·3	11·5	9·15	8	7·85
,, 20	7·5	7·5	3·55	7·2	6·5	7·15
,, 24	5·8	6·05	5	5·3	7	4·75
,, 27	6·1	11·15	4·95	6·1	7·45	6·75
Series II						
Sept. 18	10·1	10·3	4·15	7·05	7·75	8·15
,, 21	9·45	10·8	4·95	7·3	7·87	9·35
,, 25	6·05	10·8	5·15	6·1	5·85	9·25
,, 28	6·65	8·55	6·75	6·4	7·45	9·1
Oct. 2	7·65	8·5	7	8·55	8·1	8·45
Average	7·65	9·00	6·09	6·87	7·30	7·83

The average results show a rise in the threshold:

for E of 1·35 mm.
„ R „ ·78 mm.
„ M „ ·53 mm.

Taking Series I and II together (ten experiments) a rise in the threshold was found seven times with each subject. The tendency for the threshold to rise is hence distinctly greater than the tendency to fall after the work.

In the case of Series III, the spatial threshold was tested (α) before the work, (β) after the first hour's, and (γ) at the end of the two hours' work (Table VII).

Here it will be noticed that a slight rise in the threshold occurs four times at mid-day after the first hour's arithmetic. But this rise is not continued at the end of the day; on the contrary an improvement in the threshold is met with five times in a total of eight experiments.

TABLE VII.

Showing the spatial threshold before and after Work Period I and after Work Period II.

	Subject R			Subject M		
	10.45 a.m.	12.5 p.m.	4.5 p.m.	10.45 a.m.	12.5 p.m.	4.5 p.m.
	mm.	mm.	mm.	mm.	mm.	mm.
Jan. 8	9	9	6·8	11	12	11
,, 10	5·9	8·4	10·8	11·4	11·2	11·6
,, 13	8·5	10·8	10·8	11·7	11·7	11·1
,, 15	10·4	10·7	9·4	11·9	11·9	11
Average	8·45	9·72	9·25	11·50	11·70	11·17

Fatigue was undoubtedly more marked in this Series. One reason for the failure of the records to indicate it may lie in the difficulty that the subjects experienced of maintaining the same judgment with regard to the threshold for 'twoness' throughout the day. Although the subjects tried to judge the sensations as two only when there were two distinct points, it was found practically impossible to eliminate inference when the threshold was approached. It became extremely difficult to discriminate between the judgment of 'twoness' which depended on two distinct points and that which was based on the size of the area under stimulation. In a later series of experiments one of the subjects found it was distinctly more difficult to avoid inference when tired at the end of the day. That this error may arise was recognised by Prof. Binet; in reviewing Foucault's work, *L'illusion paradoxalle et le seuil de Weber*, he wrote "Ce que je crois c'est que le seuil dépend non seulement de la sensation perçue, mais aussi de la subtilité de l'interprétation, que celle-ci peut varier, et suivant les circonstances que plus l'interprétation devient subtile, plus la perception est fine, plus le seuil est petit, et plus aussi les chances d'erreur augmentent, on pourrait donc poser un grand nombre de seuils ayant chacun une proportion d'erreurs[1]."

The second object in the analysis of these records was to inquire into the correlation between the rise in threshold and the changes in speed and accuracy of the mental work. The difference between the second and the first threshold value was expressed as a percentage of the ratio of the second reading to the first. The case of the least fall in power of discrimination was ranked as first, that of the greatest fall as tenth. In Series I and II this alteration in the threshold was correlated (*a*) with the loss in speed of the mental work as determined by the number of sums in the second half-hour expressed as a percentage of the number done in the first half-hour; and (*b*) with the loss in accuracy of the mental work as determined by the number of correct answers in the second half-hour expressed as a percentage of the correct answers in the first half-hour. It was realised that with so few numbers reliable results in correlation could not be obtained. It was only expected that a general tendency might appear which could be further investigated by subsequent experiments. In Table VIII these ranks are indicated for Subject E, the smallest values in the power of discrimination denoting the lowest threshold, *i.e.* the greatest power of discrimination after the mental work. The formula used was that of the Method of Ranks as calculated by Prof. K. Pearson[1], viz.

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[1] *L'Année Psychol.* 1911, XVII. 426.

TABLE VIII.

Showing the correlation (a) between rise in spatial threshold and speed of mental work, and (b) between rise in spatial threshold and accuracy of mental work.

Subject E

Spatial threshold	Rank	Number of sums	Rank	Sq. of diff. in rank	Spatial threshold	Rank	Number of correct sums	Rank	Sq. of diff. in rank
97	3	118	1	4	97	3	119	1	4
95	2	92	8	36	95	2	57	10	64
100	4	89	9	25	100	4	91	8	16
88	1	93	7	36	88	1	109	5	16
104	6	111	4	4	104	6	114	3	9
182	10	105	5	25	182	10	106	6	16
101	5	86	10	25	101	5	84	9	16
114	7	114	3	16	114	7	115	2	25
178	9	104	6	9	178	9	105	7	4
128	8	115	2	36	128	8	111	4	16
			Total	216				Total	186

Coefficient of correlation for (*a*) = − ·313.
Probable error = ± ·2016.

Coefficient of correlation for (*b*) = − ·127.
Probable error = ± ·2196.

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[1] *Drapers' Company Research Memoirs, Biometric Series*, 1907, IV. 18; also Wm Brown, *Mental Measurement*, Cambridge, 1911, 47—53.

$$\rho = 1 - \frac{6s\,(v_1 - v_2)^2}{N\,(N^2 - 1)},$$

$$r = 2 \sin \frac{\pi}{6}\,\rho.$$

Comparison with the probable error shows that there is no certainty that a significant correlation exists between (*a*) rise in threshold and decrease in speed of multiplication, or (*b*) rise in threshold and decrease in accuracy of multiplication. This was likewise the case with the other subjects.

Fatigue as manifested by changes in muscular capacity. It might be thought that the fatigue caused by writing the answers to the sums would result in some loss of muscular power after such work. If this were so, the subject would be expected to show a clear and constant tendency in this direction, especially in Series III. Table IX gives the results before and after the hour's multiplication (Series I and II), the average shows actually an increase in muscular capacity after the multiplication work in one subject and a loss in the case of the other two; but in none of the subjects were these results constant throughout the experiments.

TABLE IX.

Showing the dynamometer records before and after one hour's multiplication in Series I and II.

	Subject E		Subject R		Subject M	
	Record :		Record :		Record :	
	Before	After	Before	After	Before	After
Series I	kg.	kg.	kg.	kg.	kg.	kg.
July 13	21	19	21	23	27	28
„ 17	18	20	20	26	28	26
„ 20	20	17	23	21	30	25
„ 24	22	16	23	20	31	27
„ 27	20	19	21	21	29·5	26·5
Series II						
Sept. 18	22	19	24	30	32	31·5
„ 21	22·5	20	22	26	29	28·5
„ 25	18	20	23	28·5	28·5	27
„ 28	16	21	21	23	29	29·5
Oct. 2	20	22	22·5	24	29	29·5
Average	19·9	19·3	22·0	24·2	29·3	27·8

TABLE X.

Showing the dynamometer records in Series III.

	Subject R						Subject M					
	10.40 a.m.		noon		4 p.m.		10.40 a.m.		noon		4 p.m.	
	left-hand	right-hand	left-hand	right-hand	left-hand	right-hand	left-hand	right-hand	left-hand	right-hand	left-hand	right-hand
	kg.	kg.	kg.	kg.	kg.	kg.	kg.	kg.	kg.	kg.	kg.	kg.
Jan. 8	15	22	16	18	15	18	26	30	22	26	28	28
,, 10	20	21	14	20	16	20·5	22	31	25	29	20	28
,, 13	15	23	20	22	16	22·5	25	28	30	31	25	32
,, 15	14	23·5	17	25	15	24	25	31	21	23	20	28

It will be noticed that the muscular capacity of the right-hand diminished three times with both subjects at mid-day, and three times with both subjects at 4 p.m. in a total of eight experiments. But as the left-hand grip diminished only once with R and twice with M, both at mid-day and at 4 p.m., such decrease, like the decrease recorded in Table IX, is not sufficiently constant for the dynamometer to serve as evidence of fatigue.

TABLE XI.

Showing the ergograph readings in Series III.

	10.50 a.m.			12.10 p.m.			4.10 p.m.		
	Total musc. performance	Number of flexions	Average height	Total musc. performance	Number of flexions	Average height	Total musc. performance	Number of flexions	Average height
Subject R	kg. m.		cms.	kg. m.		cms.	kg. m.		cms.
Jan. 8	6·18	135	2·2	5·56	109	2·4	3·22	60	2·6
,, 10	3·98	88	2·2	5·40	149	2·3	—	—	—
,, 13	4·50	96	2·3	6·92	120	2·5	7·78	208	2·5
,, 15	5·44	104	2·6	6·20	93	2·8	5·76	110	3·1
Subject M									
Jan. 8	4·66	87	2·67	6·58	148	2·2	4·02	83	2·42
,, 10	5·74	170	1·68	6·06	150	2·02	5·20	184	1·41
,, 13	5·60	182	1·53	6·02	177	1·7	6·06	164	1·8
,, 15	6·56	157	2·08	7·38	171	2·10	6·00	186	2·2

Table XI shows that the total muscular performance improved at midday with both subjects, deterioration occurring only once with Subject R. At the 4.10 p.m. test, the total performance deteriorated once with R. Unfortunately the wire broke during the second day's experiment, but the fact that improvement occurred twice at this time at least shows that mental fatigue did not cause any constant loss of muscular power in this subject. This improvement occurred both in number and in height of the flexions. Subject M showed a loss of power on three occasions at 4.10 p.m. which involved both number and height.

The results of the ergograph test, few as they are, do not encourage the belief that ergograms are capable of supplying valid evidence of fatigue for all subjects[1]. In this respect the results are in agreement with those previously found by Dr Rivers[2]. The latter has shown that even in a state of pronounced mental fatigue he could execute ergograms distinctly above the average. Dr Rivers usually obtained this result when the fatigue had been induced by interesting work, but also records an instance when the ergogram was taken after all interest had disappeared. He concludes that we have here an instance of the same association of increased muscular capacity with lowered mental activity which, there is reason to suspect, is characteristic of the effect of alcohol.

Fatigue as manifested by changes in rate of respiration.

The average results of Table XII show that the rate was slower after the mental work.

This diminution occurred in 9 of 10 experiments with E, 6 of 10 experiments with R, and in 5 of 10 experiments with M. In the case of R and M then, the respiration rate tended almost as often to increase as to decrease after the work. No correlation which was with any certainty significant could be found between the change in rate of respiration and the decrease in speed or accuracy of arithmetic of the subjects E and R. With M increase in rate of respiration was correlated with diminution in the number of right answers in the second half-hour, the coefficient of correlation being ·765 with a probable error of ± ·093.

It has been already noticed on p. 431, that decrease in the accuracy of this subject M invariably occurred in the second half-hour, and could be regarded as affording some estimation of her condition of fatigue. In the light of that fact the present correlation must be interpreted as indicating that for this subject increase in rate of respiration bears some relation to fatigue, although, as was seen in Table XII, there is no constant tendency for the rate of respiration to increase after mental work.

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[1] Further experiments with school children also served to discourage this belief. Cf. *J. of Educ. Psychol.* 1911, II. 144, 145.

[2] *The Influence of Alcohol and other Drugs on Fatigue*, London, 1908, 103—105.

TABLE XII.

Showing the number of respirations per minute, before and after one hour's multiplication.

	Subject E		**Subject R**		**Subject M**	
	Record:		**Record:**		**Record:**	
	Before	**After**	**Before**	**After**	**Before**	**After**
Series I						
July 13	22·90	15	18·6	16·5	18·5	15
„ 17	19·1	18·8	16·6	17·1	16·4	19·1
„ 20	16·8	15	15·9	15·7	16·4	17·5
„ 24	17·7	17·5	16·3	16·5	18·3	18·8
„ 27	18·9	19·2	19	18·4	16·8	16
Series II						
Sept. 18	20	16·9	16·5	15·6	16·9	18·7
„ 21	18·4	13·3	16·8	18·4	19·5	20·3
„ 25	17·5	17·4	17·3	17·5	18·7	16·9
„ 28	16·9	11·3	15·6	15·4	20	19·5
Oct. 2	12	11·7	18	15	20·7	15·3
Average	18·02	15·61	17·06	16·61	18·22	17·71

Fatigue as manifested by change in the rate of the pulse before and after mental work.

The average results for each individual given in Table XIII show a rise in the case of one subject, a fall in the case of the other two.

A fall occurred in
7 of 10 experiments with E (once equal),
8 „ 10 „ „ R,
and in 4 „ 10 „ „ M.

This retardation may be due to the persistence of the slower rate which, according to Binet's[1] results, occurs during mental work. It should be noticed that with each subject, increase only occurs on those days when before work the pulse was below 70. The one exception to this is in the case of R where there is a rise from 75 to 80. Such increase in rate may indicate the effect of work as a general stimulant.

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[1] *La Fatigue Intellectuelle*, Paris, 1898, 97.

TABLE XIII.

Showing record of the pulse rate per minute, before and after one hour's multiplication.

	Subject E		Subject R		Subject M	
	Record :		Record :		Record :	
	Before	After	Before	After	Before	After
Series I						
July 13	75	75	93	84·7	75	60
„ 17	78·9	78	78	75	78	72
,, 20	65·75	78	75	80	78	62·5
,, 24	78	68·25	78·5	73·5	68·25	81
,, 27	78	64·8	78	64·8	66·3	66
Series II						
Sept. 18	75·7	69	75	63·1	69	78
,, 21	75	66	65	63·1	66	78·7
,, 25	78	64·5	68·25	69	64·5	68·25
,, 28	66	73·5	78	76·5	66	73·5
Oct. 2	78	69	78	69	62	72
Average	74·83	70·60	76·67	71·87	69·30	71·19

No significant correlation could be found either between decrease in the rate of the pulse and decrease in speed of working or between decrease in the rate of the pulse and decrease in accuracy of working.

Fatigue as manifested by changes in accuracy and speed of perception.

A decrease was observed

4 times in 10 experiments with Subject E,
8 „ „ 10 „ „ „ R,
and 8 „ „ 10 „ „ „ M.

R had chosen and arranged the patterns before the series began; and had selected those that appeared to her consistent and equal in every respect. For this subject, the test was nevertheless a satisfactory one.

TABLE XIV.

Showing the number of correct units drawn before and after multiplication work.

	Subject E		Subject R		Subject M	
	Record:		Record:		Record:	
	Before	After	Before	After	Before	After
Series I						
July 13	177	193	298	293	324	306
„ 17	289	257	427	358	435	370
„ 20	243	252	402	318	385	319
„ 24	221	274	320	234	231	228
„ 27	264	250	339	350	410	401
Series II						
Sept. 18	287	246	371	349	342	254
„ 21	220	299	294	338	269	384
„ 25	342	301	429	333	373	359
„ 28	284	286	386	322	333	298
Oct. 2	243	303	367	339	294	305
Average	257·0	266·1	365·3	328·4	334·6	322·4

No significant correlation could be found between the change in the accuracy and speed of the work and change in the number of correctly finished units after the arithmetic.

Conclusions from the tests for fatigue.

According to the above results, fatigue was manifested in the following manner:

(*a*) In the case of E, by diminution in the rate of pulse and respiration and possibly by diminution in discriminative power as determined by the cutaneous spatial threshold.

(*b*) In the case of R, by diminution in the rate of pulse and in accuracy and quickness of perception.

In the case of M, with the single exception of rate of respiration and accuracy of arithmetic, no correlation which was with any certainty significant could be found between decrease in accuracy or output of mental work and the subject's behaviour to the tests.

V. GENERAL CONCLUSIONS.

From an analysis of the multiplication work and from the results of the tests applied, we may conclude that the signs of fatigue differ according to the individual; that the response which the individual makes in the first stages of fatigue depends on the stability of dispositions previously acquired, on the method of working (see introspective notes in Appendix), and on the present condition of the subject. Consequently a numerical estimation of the decrease in speed or in accuracy will not invariably represent the extent of fatigue; and the changes in spatial threshold, in rate of respiration, in pulse rate, and in muscular capacity, are not sufficiently constant to be regarded as sure signs of fatigue in its early stages.

VI. APPENDIX.

INTROSPECTIVE NOTES MADE IN THE MULTIPLICATION TESTS.

Subject E. July 13. "Some parts of columns found easier than others. Same three numbers recur but are not recognised until too late, 48 to be multiplied worries me very much; carriage and ten figures of first two factors are constantly lost and the sum re-done."

July 17. "Found the products easier and recognised the same product, *e.g.* 9 × 8 × 7, in various forms. Very tired at end."

July 20. "Notice recurrence of numbers again and again. Learn that 7 × 8 × 9 in any order = 504. Dread the appearance of 48 as product of numbers."

July 24. "Several of the products are becoming familiar. At beginning of hour felt quite fresh, but very dull at close."

July 27. "Found work easier, results familiar and thus no checking is required."

Sept. 18. "All the old products forgotten, but as the work proceeds some become familiar; the task seems very hard."

The fact that E wrote of repeating and checking the working of sums would denote a habit of accuracy which became more firmly established as the products grew familiar and written without fear of mistake. This supposition was confirmed by the very small percentage of faults found in her work.

Subject R. July 13. "Over the more difficult calculations, thoughts quite unconnected with the calculations continually obtruded."

July 17. "Easier to concentrate than time before."

July 20. "Found the ten minutes' pause made it more difficult to start again and did not feel any benefit from the pause."

July 24. "As fatigue came on found that the sum to be multiplied was not visualised as it had been earlier, that more and more the ten figure was forgotten and the product had to be again obtained. Some of the figures becoming familiar; feeling tired."

Sept. 18. "Could not visualise first figures; could not remember what figure to carry or what the ten figure of the first product was."

Sept. 21. "Could visualise better both product and final result. When getting tired found there was a tendency to articulate names of figures."

With this second subject it appears that two changes might take place on the onset of fatigue. To obtain a more distinct visual memory image more time might be spent in impressing the first product on the mind, *i.e.* by articulation, in which case the work would be slower though still accurate; or else the image being less vivid might be forgotten, in which case the accuracy would diminish but not necessarily the speed.

Subject M. July 17. "Very difficult to concentrate thoughts on multiplication; products often written without thinking, almost mechanically, feeling very tired during last half-hour."

July 24. "Multiplication easier, but thoughts constantly wandered. None of the products recognised as having occurred before."

Sept. 18. "The work seemed especially hard; each sum a matter of labour; conscious of very little visual imagery, mostly verbal."

Sept. 21. "Felt I was working more quickly, though products did not become familiar; frequently said $6 \times 12 = 54$ without knowing it was wrong until too late, felt very tired."

Subject M could never depend on the memory of any product, and it was noticed that the mistakes were specially frequent in the product of the first two figures. There was no evidence of any stability in the habit of accuracy.

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ON MEMORY AND THE DIRECTION OF ASSOCIATIONS.

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I. Introductory.
II. Experimental.
III. Theoretical and Conclusions.

I. INTRODUCTORY.

IT seems to be generally held by psychologists and physiologists, sometimes on *a priori*, sometimes even on experimental grounds, that the direction of association as revealed by ecphory[1] is predominantly in the forward direction. If *e.g.* A—B—C have been experienced in this order, and B be given at any future time, then C is 'ecphored' in preference to A in the vast majority of cases.

The belief in the forward direction of ecphory is confidently asserted even by some who maintain the forward direction of the formation of associations to be apparent only. I will quote, as an example of physiological theory not based on experiment, that of Semon. According to him[2], "successive association is merely a result of simultaneous association....To each original stimulus there corresponds a synchronous [nervous] excitation. But with the cessation of the stimulus this excitation does not suddenly cease; it is not interrupted sharply, but disappears gradually (*klingt allmählich ab*), occasionally showing one or more fluctuations (cf. after-images) during this disappearance. The excitation during this phase of disappearance has been called the akoluthic excitation. Finally we must bear in mind that the excitation acts engraphically not only during its stronger synchronous phase, but also during its weaker akoluthic phase.

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[1] I adopt this term, coined by R. Semon, as strictly technical and less given to ambiguity than *e.g.* revival, reproduction, etc. Cf. Richard Semon, *Die Mneme*, 2te Auflage, Leipzig, 1908.

[2] *Loc. cit.* 133 *seq.*

"In a succession of discontinuous stimuli the synchronous excitations, which we will call C^s, D^s, E^s, F^s, do not of course occur simultaneously, but with them the synchronous excitation D^s there does coexist the passing akoluthic phase of its predecessor, which we will distinguish in this phase as c^a, or, to indicate its progressive diminution from moment to moment, as c^{a1}—c^{a2}—c^{a3}—0. The following table makes the simultaneous relations of the synchronous phase to the akoluthic phases of its predecessors quite clear.

$$
\begin{array}{lccccccccccccccc}
\text{Phase} & 1 & & 2 & & 3 & & 4 & & 5 & & 6 & & 7 & & \\
 & C^s & — & c^{a1} & — & c^{a2} & — & c^{a3} & — & 0 & & & & & & \\
 & & & \mid & & \mid & & \mid & & & & & & & & \\
 & & & D^s & — & d^{a1} & — & d^{a2} & — & d^{a3} & — & 0 & & & & \\
 & & & & & \mid & & \mid & & \mid & & & & & & \\
 & & & & & E^s & — & e^{a1} & — & e^{a2} & — & e^{a3} & — & 0 & & \\
 & & & & & & & \mid & & \mid & & \mid & & & & \\
 & & & & & & & F^s & — & f^{a1} & — & f^{a2} & — & f^{a3} & — & 0
\end{array}
$$

"We find that the synchronous excitation D^s is simultaneously associated with the akoluthic excitation c^{a1}, the synchronous E^s with the akoluthic d^{a1} and with the much weaker c^{a2}, the synchronous excitation F^s with the akoluthic e^{a1}, with the much weaker d^{a2} and the vanishing c^{a3}. In this manner we derive successive from simultaneous association, and bring to light, as a necessary sequel, the more special fact, experimentally demonstrated by Ebbinghaus, that not only is there successive association between the members of a series that immediately follow one another, but that each member is also, although more feebly, associated with the second following and still more feebly with the third following, possibly also with the fourth following. A limit to the formation of such remoter associations is only set by the continuous diminution and final complete extinction of all akoluthic excitations. This also explains why a series of excitations can only give rise to associated engrams when during the original excitation the time intervals between the separate members have not been so long that the akoluthic excitation of the preceding member has already vanished when the synchronous excitation of its successors begins....

"That simultaneously produced engrams are linked with equal force in both directions, whilst this is not the case with successively produced engrams, constitutes a difference of fundamental importance. We arrive at this statement of difference from the fact that if engram *a* is associated simultaneously with engram *b*, then under ordinary circumstances the ecphory of *a* acts just as strongly, but not more strongly, ecphorically on *b*, than the ecphory of *b* on *a*. *If, however,* a *and* b *are successively associated, then without exception the ecphory of* a '*ecphores*' b *with incomparably greater strength than* vice versa[1]."

To find a plausible explanation for this supposed fact Semon encounters great difficulty; for it is not easy to see why if, *e.g.*, D^s is given again, the engram E^s (*via* d^{a1}) rather than that of C^s (*via* c^{a1}) should be ecphored. He falls back upon the cyclicly recurring sensations derived from our body which continuously occur in one direction only; and these, he says, form a background upon which our other experiments are, as it were, embroidered[2].

Ebbinghaus[3] and Müller and Schumann[4] have shown by experiment (and Müller and Pilzecker[5] have confirmed these results) that associations between successive experiences have a distinctly stronger tendency to forward direction in ecphory. Thus, Müller and Pilzecker found[6] that when pairs of syllables had been learned as trochees and the first or accentuated syllable was shown, then in 50°/$_0$ of cases the other syllable of the pair, *i.e.* the succeeding syllable was reproduced, whilst when the second or unaccentuated syllable was given the first member of the pair was ecphored in only 38°/$_0$ of the answers. There was also a shorter reaction-time for forward than for backward directed ecphory.

Although, then, previous experimental results do not warrant so sweeping a hypothesis as that of Semon's, yet they show that, as stated above, the tendency of ecphory and therefore that of association, is mainly in the forward direction.

II. EXPERIMENTAL.

An investigation into certain problems of association led me to include an examination of the above problem. I had been working with material different from Müller's, viz. with colours and diagrams, when I came to doubt the universal applicability of the results of this investigator. As also my methods of experiment were different from Müller's, I decided to work in double series, viz. with diagrams in one, and with nonsense-syllables in the other, in order to ascertain whether any divergency in results was due to difference in material or to difference in procedure.

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[1] The italics are mine.

[2] R. Semon, *Die mnemischen Empfindungen*, Leipzig, 1909, 198 *seq.* Perhaps the most inconvincing chapter in the otherwise decidedly suggestive and clever book.

[3] *Ueber das Gedächtniss*, Leipzig, 1885.

[4] G. E. Müller and Schumann, "Experimentelle Beiträge zur Untersuchung des Gedächtnisses," *Ztsch. f. Psychol. u. Physiol. d. Sinnesorg.* 1894, VI.

[5] G. E. Müller and A. Pilzecker, "Experimentelle Beiträge zur Lehre vom Gedächtniss," *Ztsch. f. Psychol.* 1900, *Ergänzungsbd.* I.

[6] *Loc. cit.* 208.

The first group of experiments which I shall describe, was distinguished by me as group 4. It consisted of two smaller groups, group 4a in which diagrams were the material, and group 4b in which I worked with nonsense-syllables.

In *group 4a* a continuous series of eight simple diagrams was presented to the observer without any interval by means of a Müller's Memory-apparatus. There was no gap to mark a beginning or an end to the series. The time of exposure of each diagram was ·5 sec. It was found advisable to regulate the number of presentations of a series by the degree of the completion of the learning; in this way it was generally possible to avoid cases where the subject made no scores and also cases where all answers were correct. Both of these extremes, of course, would have failed to throw any light on the problem under investigation. The subject was told to tap on the table when he thought he knew, *i.e.* could reproduce, the series. For those subjects who found the learning very fatiguing twenty exposures of the series were given followed by an interval of rest for three minutes, and this alternation continued until the subject announced by tapping on the table that he thought he knew the series.

After a series of diagrams of group 4a, a series of nonsense-syllables of *group 4b* was read aloud and learnt in a similar manner. This series consisted of eight syllables chosen with the usual precautions, and was made up and presented exactly as the series of group 4a.

Then the examination of the subject with the diagrams was proceeded with, followed by the examination with the syllables. On another day the order of learning the diagrams and syllables was reversed. For the purpose of examination the subject was seated at a table in front of a large screen which hid the operator sitting on the opposite side of the table. A preparatory signal was given and then a syllable or diagram of the series learnt was exposed in a window in the screen. The subject reacted by saying "now." The reaction-time was measured by means of a stop-watch registering ·2 sec. A list on which the diagrams were arranged haphazard (or the syllables in alphabetical order) was then shown, and the observer picked out, without naming, the one that had come into his mind, stating how it appeared 'related' to the diagram (or syllable) shown. The observer was allowed to pick out any second diagram (or syllable) that had also come into his mind.

The following instructions, which were given to the observer, will serve to make the experiment quite clear:

Instructions before learning. You will be shown a successive and continuous series of diagrams (or nonsense-syllables).

You are required to learn these; you continue learning until you think you know the series; then tap on the table.

With the diagrams avoid naming or artificial associations.

Learn the syllables in the manner most natural to you, *i.e.* you may pronounce them aloud, or inwardly if you think you learn them better that way[1].

In all cases avoid rhythm or grouping in pairs or threes, etc., if possible, and avoid all artificial helps and associations.

Instructions before examination. A diagram (or syllable) of the series you have learnt will be shown you and you are required to react as soon as a diagram (or syllable) comes into your mind, that appears to you *related* to the one shown. By 'related' is meant a diagram (or syllable) immediately preceding or immediately succeeding the one shown, or even if it occupies a place next but one to it. If a diagram (or syllable) is further removed than this, we should not call it related. As there are several related diagrams (or syllables) *you must not wait* until several or all have been recalled, but *react at once*, as soon as the first one comes up.

It is perhaps needless to say that I always made sure that the instructions were clearly understood by the subject.

In marking the scores the first succeeding member was counted as '1 forwards,' and the second succeeding as '½ forwards'; the immediately preceding one was counted as '1 backwards,' and the one before this as '½ backwards.' If second answers were given as to a diagram or syllable which was thought to be related in the other direction, they were counted separately on the same basis. There were four series in each group and the experiments were performed on seven observers.

Table I gives the absolute numbers of scores of first answers 'forwards' and 'backwards' in group 4 and Table II those of the scores of the second answers.

In Table III are given the median reaction times in seconds and their averages. For the present measurements the median is a more reliable characteristic than the average.

On looking at Table I we find that there seems on the whole a greater tendency to the forward direction in ecphory with syllables than there is with diagrams. With the latter the *backward* direction is

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[1] The method invariably adopted by all subjects was that of pronouncing the syllables.

TABLE I.

Group 4. Absolute numbers of scores. (First answers.)

Subject	4 *a*. Diagrams		4 *b*. Nonsense-syllables	
	forwards	backwards	forwards	backwards
A	17½	13½	7½	6
C	12	7	10	7½
F	18½	7½	17½	8
G	14½	12	11	6
H	9½	11½	12½	3
R	12	13½	14½	9
W	10	13	11½	7½
Total	94	78	84½	47
Average	13·5	11·1	12·1	6·7
Average °/₀	42 °/₀	34·8 °/₀	37·7 °/₀	21 °/₀
Proportion	1·3 : 1		1·7 : 1	

TABLE II.

Group 4. Absolute numbers of scores. (Second answers.)

Subject	4 *a*. Diagrams		4 *b*. Nonsense-syllables	
	forwards	backwards	forwards	backwards
A	11½	16	1	½
C	5	3½	1	3½
F	3	12	5½	10½
G	7	6	3	6
H	7½	4	1½	3
R	2	6½	6	4½
W	4	2½	3½	2
Total	40	50½	21½	30
Average	5·7	7·2	3·1	4·3
Average °/₀	17·8 °/₀	22·5 °/₀	9·6 °/₀	13·4 °/₀
Proportion	1 : 1·3		1 : 1·4	

TABLE III.

Group 4. Median reaction-times. (First answers.)

Subject	4 *a*. Diagrams		4 *b*. Nonsense-syllables	
	forwards	backwards	forwards	backwards
A	2·2	4·0	4·0	5·0 sec.
C	4·4	5·5	5·7	7·1 ,,
F	8·2	8·0	9·0	13·2 ,,
G	2·7	2·6	5·0	4·2 ,,
H	4·8	4·8	5·5	5·2 ,,
R	2·4	2·2	2·4	2·2 ,,
W	2·9	2·7	3·4	4·6 ,,
Average	4·0	4·3	5·0	5·9 sec.

stronger with three of the seven subjects, whilst with syllables the *forward* direction is stronger with them all. The averages are approximately: for diagrams—'forwards' 13½ scores, 'backwards' 11 scores; for syllables—'forwards' 12 scores, 'backwards' 7 scores; or, expressed in proportions, for diagrams—'forwards' : 'backwards' scores :: 1·3 : 1; for syllables—'forwards' : 'backwards' scores :: 1·7 : 1. The probable errors, however, are too high in comparison with the observed differences to allow much stress to be laid on them, being 21 for the diagrams and 13 for the syllables. We require therefore further evidence before we can draw any conclusions.

Table II contains, as we have seen, the absolute numbers of scores of the second answers. Now if, say, *A*—*B*—*C* are three consecutive members of any series, and *B* be given as a stimulus during the examination and the subject reacted by a first answer with *C*, then *A* would be expected as the second answer. That is to say, the ratio of the forward scores to the backward ones in the second answers would be expected to be the inverse of this ratio in the first answers; for if the backward and forward associations are of equal strength and the first answer was, say, a forward association, then the second answer ought to be a backward association. We find this to be the case with the diagrams, but with the syllables it is not so, the ratio in Table II being only 1 : 1·4 instead of 1 : 1·7.

The result of Table II then tends to confirm the inference that might be drawn from Table I, viz. that the forward direction in ecphory is less with diagrams than with syllables.

The evidence of Table III, so far as it goes, is in the same direction. We see that the difference between the reaction-times of forward and backward scores is greater with syllables than with diagrams. For while forward scores in these latter are ·3 sec. shorter than the backward scores, the difference rises to ·9 sec. with syllables.

As I had reason to believe that these results were vitiated by an unconscious bias, probably varying in different individuals, to *set* the ecphory in the one or other direction[1], I performed another series of experiments which I will designate *group* 5. As these experiments were made after a considerable lapse of time I utilised the same material as in the experiments of group 4. There were four subjects, with two of whom (F_1 and C_1) the experiments were repeated after an interval of several months (F_2 and C_2). The instructions for learning were exactly the same as in group 4; so also were those given just before the examination, see p. 451, with the following addition:

> "Try and let the first diagram (or syllable) be the preceding (or succeeding) one; but do not let this instruction interfere with your reaction and react at the very first related diagram (or syllable) whether or not it seems in the desired direction."

That is to say, I tried to create a prescribed forward or backward tendency in the direction of the ecphory.

Table IV gives the absolute number of scores (the marking was exactly the same as before), and Table V the median reaction-times and their averages.

The results given in Table IV confirm the anticipation that there can exist a tendency to react in a certain direction, possibly varying more or less at different times with the same observer, and that such tendency may well have been one of the factors that were responsible for the experimental results of group 4.

Accordingly, the experiments of group 5 are of threefold value. They show

(1) that the already suspected factor of an unconscious bias to react

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[1] By this I mean that, if a subject had *e.g.* learnt a series *A*—*B*—*C*, etc., and if *B* was given in the examination, instead of endeavouring to 'ecphore' any related member (*i.e.* *A* or *C*) impartially, the subject more or less unconsciously set his attention either in the forward direction, *i.e.* to the succeeding members *C*, etc., or in the backward direction, *i.e.* to the preceding members *A*, etc. If such an unconscious setting of attention had been a vitiating factor in the experiments described, then, I argued, it could be increased by giving definite instructions as to the direction of the ecphory just before the examination. During the learning the subject was, of course, absolutely ignorant which direction he was to adopt.

TABLE IV.

Group 5. Absolute numbers of scores. (Direction prescribed.)

Subject	5 *a*. Diagrams				5 *b*. Syllables			
	Directed forwards		Directed backwards		Directed forwards		Directed backwards	
	forwards	backwards	forwards	backwards	forwards	backwards	forwards	backwards
F_1	10½	5	3	12	6	4	6	7½
F_2	11	3	3½	11½	10½	3½	8	6
C_1	7½	2½	4	9	14	—	12	1½
C_2	11	1	3	9	9½	1½	12	3
R	15	1	2½	9½	12	4	5	8½
G	5	3½	5½	6½	4½	3½	6½	4
Total	60	16	21½	57½	56½	16½	49½	30½
Average	10	2·7	3·6	9·6	9·4	2·8	8·2	5·1
Average °/o	62·5 °/o	16·7 °/o	22·4 °/o	59·9 °/o	58·9 °/o	17·2 °/o	51·6 °/o	31·8 °/o
Proportion	3·8 : 1		1 : 2·7		3·4 : 1		1·6 : 1	

TABLE V.

Group 5. Medium reaction-times. (Direction prescribed.)

Subject	5 *a*. Diagrams				5 *b*. Syllables			
	Directed forwards		Directed backwards		Directed forwards		Directed backwards	
	forwards	backwards	forwards	backwards	forwards	backwards	forwards	backwards
F_1	6·2	5·0	9·0	6·8	5·5	9·2	7·8	9·8
F_2	4·8	6·4	3·5	6·0	4·2	9·5	2·6	4·5
C_1	4·8	4·8	6·9	6·5	5·8	—	4·9	5·6
C_2	3·0	4·9	4·9	3·7	3·0	9·4	2·4	5·0
R	2·2	3·0	3·6	3·0	1·5	2·3	2·0	3·0
G	2·2	1·8	1·8	1·4	3·6	2·0	2·2	1·5
Average	3·9	4·3	4·9	4·6	3·9	6·5	3·7	4·9

in a certain direction is possible (although there may be other disturbing factors at work);

(2) that diagrams are much more susceptible to this tendency than syllables, that is to say, that the direction of association is much more plastic with diagrams than with syllables;

(3) that the results of the experiments of group 4 would have been more striking if the above-mentioned factor could have been eliminated.

In the experiments of group 4 we found that the forward direction of association is decidedly more marked with syllables than with diagrams. In those of group 5 we see that, whereas the backward associations are quite easily ecphored in the case of diagrams, the process is much more difficult in the case of syllables.

The reaction-times[1] (Table V), as far as they go, seem to indicate that, in the case of diagrams, with a forward-directed tendency in ecphory the forward scores have a shorter reaction-time than the backward scores; but with a backward-directed tendency in ecphory the backward reaction-times are the shorter. In the case of syllables, however, we find that both with a forward and a backward direction in ecphory the backward scores have always a longer reaction-time. To this extent then the reaction-times confirm the results of Table IV, viz. that syllables have a greater tendency to forward ecphory than diagrams.

The experiments just described are, as I have said before, complicated by various factors not only in the learning but also in the ecphory. But on the whole they prove that the associations formed in the learning of syllables have a very strong forward tendency, which is lacking in the case of those formed in the learning of diagrams. This fact is still more convincingly demonstrated by the results of the experiments of group 3, where, I believe, I have been successful in eliminating the disturbing factors of group 4 to a large extent. *Group 3a* consisted of four series, each of six pairs, which were repeated after the lapse of a certain time. Each pair was made up of a colour and a diagram which were presented successively at such a rapid rate (each exposure was as before only ·5 sec.) that there was not the least possibility of the conscious formation of backward connexions; that is to say, whilst the second member of a pair was present there was not time enough for the subject to think again of the first member. Colours formed as often first members of pairs as did the diagrams. This mixed material was chosen as it was better suited for the formation of pairs than colours or diagrams alone. To differentiate the pairs successfully one from another, the following device was adopted. Each colour or diagram occupied only half the field of the window of Müller's Memory-apparatus; the members of any one pair occupied say the left, and those of the next pair followed in a similar manner on the right side of the field. The accompanying figure, showing part of an endless band on which a series was presented, will make this arrangement quite clear. To each subject the four series were presented on two different days, and the whole experiment was repeated after the lapse of some months; so that 48 answers were obtainable from each subject.

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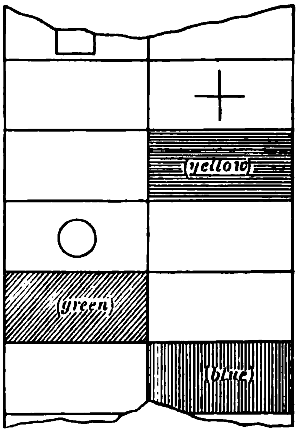
[1] For the paradoxical behaviour of reaction-times *vide* Müller and Pilzecker, *Die Lehre vom Gedächtnis*, Leipzig, 1900, 32 *seq.*



The following were the instructions given to each subject:

> Sit down on the chair at such an easy distance from the apparatus that you can well see the window.
>
> You will be presented in the window in rapid succession with a series of pairs consisting each of a coloured field and a black diagram. The members of each pair are given successively; sometimes the coloured field precedes the diagram, sometimes the diagram is given first. The pairs are differentiated by appearing alternately at different sides of the window, *e.g.*, there appears in the left half of the window first a coloured field which is succeeded by a diagram, or *vice versa*, whilst the right half of the window is empty. The next pair will then appear in the right half in a similar manner, whilst the left half remains empty, and so on.
>
> You are asked to learn these pairs by attending to them in such an attitude of mind as to take in the sense-presentation avoiding to form any verbal or other artificial association than that given directly by the presentations. Thus, *e.g.*, if the colour of the field is green, do not try to memorize by calling it green, or if an octagon were shown you as a diagram, do not try to formulate its name, but simply take in the sense-presentation.

Do not try to remember the coloured field and the diagram by any artificial association, *e.g.*, if you were shown an arrow followed by a red field: 'arrow draws blood—blood red.' Avoid any such or similar associations.

After the series has been presented to you a certain number of times, the window will be closed. Cease at once to think about the experiment. If you find the effort too great, take up immediately the copy of *Punch* provided and read in it, giving yourself up entirely to your reading and banishing any thought of the experiment.

You will then be shown a colour or a diagram alone. If a colour is given, look out for the diagram called up by it; if a diagram is given, look out for the colour associated with it. Avoid using any names for diagrams or colours either aloud or mentally.

As soon as you have a definite result, say "Now"; or as soon as you think that you cannot give an answer, say "Nothing."

You will then be shown a collection of diagrams or colours respectively and you are requested to point out, without naming, the diagram or colour called up and to state how they appear related to each other. In doing so, state whether you feel certain, doubtful, very doubtful, etc. If more than one colour or diagram were called up, point them all out and give details.

Record any further points revealed by introspection, but do not sit down with the intent to introspect.

During the interval between the several examinations avoid thinking about the experiment and, if necessary, occupy yourself by reading *Punch*.

The examination was similar to that described in the case of group 4. Only one member of each pair was shown as stimulus; but of three pairs the first member, and of the three other pairs the second member was shown. It was also arranged that colours and diagrams were shown an equal number of times. This group of experiments had none of the drawbacks of group 4. The answer was here either right or wrong and the associations were formed almost exclusively between the members of each pair, whilst in group 4 each member of the series also formed associations backwards with preceding members of the series and forwards with succeeding ones.

In connexion with this group 3a the experiments of *group 3b* were conducted. It consisted of the same number of series as group 3a, but each series contained six pairs of nonsense-syllables, which were presented to the subject, read aloud and learnt by him in a similar manner as in group 3a. The series of these two groups, 3a and 3b, were taken alternately. The instructions for learning and the mode of examination were similar to those in group 3a.

The results are given in the following tables. Table VI gives the absolute number of scores, their averages and proportions, and Table VII gives the median reaction-times and their averages. A right answer was counted as 1, and if two answers were given, one of which was right, it was counted ½. With each subject a total of 24 scores forwards and 24 backwards was possible in each of the two groups.

TABLE VI.

Group 3. *Absolute numbers of scores.*

Subject	3 *a*. Pairs of colours-diagrams		3 *b*. Pairs of syllables	
	forwards	backwards	forwards	backwards
A	15	21	12	5
C	19½	18	4	1
F	21	20	14	5
G	11	11	14	7
H	17	16	10	6
R	21	18	12	9
W	11½	18	14	7
Total	116	122	80	40
Average	16·6	17·4	11·4	5·7
Average %	69·2 %	72·5 %	47·5 %	23·8 %
Proportion	·95 : 1		2 : 1	

TABLE VII.

Group 3. *Median reaction-times.*

Subject	3 *a*. Pairs of colours-diagrams		3 *b*. Pairs of syllables	
	forwards	backwards	forwards	backwards
A	2·0 sec.	1·4 sec.	1·3 sec.	1·8 sec.
C	2·1 ,,	3·9 ,,	3·3 ,,	10·0 ,,
F	3·8 ,,	3·4 ,,	3·0 ,,	4·0 ,,
G	3·1 ,,	1·2 ,,	2·2 ,,	1·4 ,,
H	2·4 ,,	1·6 ,,	4·1 ,,	4·4 ,,
R	1·4 ,,	1·8 ,,	1·5 ,,	1·6 ,,
W	2·2 ,,	1·7 ,,	2·6 ,,	1·9 ,,
Averages	2·4 sec.	2·1 sec.	2·6 sec.	3·6 sec.

From Table VI we see that whereas *the associations formed between syllables are*, as Müller and Pilzecker found them to be, *predominantly forward* (in the forward direction twice as strong as in the backward direction), *the associations formed between colours and diagrams are equally strong in the two directions*. The slight predominance which the figures show in the case of the backward direction arises from causes lying obviously within the limits of experimental error.

Table VII, too, which gives the median reaction-times and their averages, shows the approximate equality of both forward and backward associations of colours-diagrams, viz. 2·4 and 2·1 secs. respectively, whilst with syllables the backward associations require a longer time for ecphory than the forward associations, viz. 3·6 secs. as against 2·6 secs.

For reasons which will be made apparent in the theoretical part of this paper, I was induced to make another set of experiments, viz. those of *group* 7. These are a modified form of experiments of group 3b. The experimentation was in all respects exactly the same as that in group 3b with the exception that the subject had to pronounce aloud the word 'ten' every time a nonsense-syllable appeared in the window of the Müller Memory-apparatus in which the series was presented. I tried to get the same observers as I had had in the experiments of group 3, but

TABLE VIII.

Group 7. Absolute numbers of scores.

Subject	forwards	backwards
A	16	11
C	16	10
F	13	15
H	8	6
K	6	9
P	15	18
R	11	13
W	13	10
Total	98	92
Average	12·2	11·5
Average °/o	51 °/o	47·9 °/o
Proportion	1·06 : 1	

owing to the fact that these new experiments were done in the following academic year I was only partially successful. Table VIII gives the absolute numbers of scores (like Table VI of group 3) and Table IX the median reaction-times (cf. Table VII).

TABLE IX.

Group 7. Median reaction-times.

Subject	forwards	backwards
A	1·8 sec.	5·6 sec.
C	3·6 „	3·8 „
F	6·2 „	5·8 „
H	3·8 „	3·4 „
K	4·6 „	3·6 „
P	2·0 „	2·0 „
R	2·2 „	1·8 „
W	2·0 „	2·7 „
Average	3·3 sec.	3·6 sec.

If we now compare the two tables with Tables VI and VII we find the proportions to be as follows:

	Group 3*a* Colours-diagrams	Group 3*b* Syllables learnt aloud	Group 7 Syllables learnt visually
Forward	·95	2	1·06
Backward	1	1	1

It is evident that with the suppression of the motor factor nonsense-syllables show a behaviour quite different from that when the motor factor is not suppressed, and we may infer that the more successfully this is done the more the syllables approach in behaviour to colours and diagrams.

This is true not only of the direction of associations but also of their reaction-times, which stand as follows:

	Group 3*a* Colours-diagrams	Group 3*b* Syllables learnt aloud	Group 7 Syllables learnt visually
Forwards	2·4 sec.	2·6 sec.	3·3 sec.
Backwards	2·1 „	3·6 „	3·6 „

In syllables learnt aloud, *i.e.* where the motor factor is active, the backward reaction-time is one second longer than the forward reaction-time, whilst in colours and diagrams and syllables learnt visually, *i.e.* where the motor factor is suppressed, the two reaction-times are much more equal.

I wish to thank all those who kindly assisted me in this research. I am grateful to Professor Spearman for allowing the work to be carried out in his laboratory and for his assistance and advice; to Dr Aveling, Miss Carey, Mr Flügel, Mr Gerrard, Miss Hember, Mr Kay, Mr Paton, Miss Rand, and Miss Williams for having acted as observers.

III. THEORETICAL AND CONCLUSIONS.

We have seen that *the experimental results given above are*, as far as colours and diagrams are concerned, *against the prevailing physiological and psychological doctrines.* The so-called 'law of forward conduction,' as will be shown more clearly later, has been derived primarily from physiology; the psychological experiments by which it was further supported were made almost entirely with nonsense-syllables. As my methods of experiment were different from those of the classical researches of Ebbinghaus, Müller and others, I performed control experiments with nonsense-syllables along with my principal experiments in which colours and diagrams were the material. These control experiments confirmed the results obtained by these investigators and therefore show that my results with colours and diagrams are not due to any difference of procedure but only to difference of material. Hence it would appear, that *the conclusions drawn from the data previously obtained require revision*; they seem to apply not to memory in general, but only to the memory of syllables and words.

In order to account for these results, let us now examine how my experiments with syllables and with colours and diagrams differed from one another. Every care had been taken that the experimental methods used with these two kinds of material should be similar save in this respect: In the case of syllables pronunciation aloud was permitted and invariably adopted; on the other hand, in the case of colours and diagrams the subjects were asked sedulously to avoid any naming, both while learning and while being examined. It is evident from this that *in learning the syllables visual memory, auditory memory, and motor memory were involved.* During the examination distinct visual images were present in most subjects, auditory images were also quite evident in most, and above all motor images (or motor sensations) were present in every one of the subjects and were very pronounced[1]. No statements were made by any observer which indicate an imageless reproduction of the nonsense-syllables. On the other hand, *in learning the colours and diagrams only visual imagery and,* from certain introspective results, *in some subjects imageless reproduction*[1] *were concerned.* There was no trace of auditory or motor images.

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[1] In the protocols such statements are frequent: "Auditory and motor images very marked," "Impossible to prevent auditory and motor imagery," "Reproduced word came usually auditory and motor, faint visual image," etc.

Hence the memory concerned in learning the syllables contained a motor element which was absent in that concerned in learning the colours and diagrams. For reasons which will appear presently, I was led to believe that this motor element was responsible for the difference of behaviour observed, and that if it could be eliminated, the syllables would behave exactly like the colours and diagrams. The procedure in group 7 experiments was therefore designed to inhibit, as much as possible, the utterance of the syllable by making the subject pronounce the word 'ten' every time a syllable appeared in the window of the apparatus. It was found that—although the auditory image remained comparatively strong—just in proportion as the motor element approached complete elimination, the results became identical with those obtained from the colours and diagrams. Hence it is evident that the difference of behaviour was due solely to the motor element, and that all the other elements (visual, auditory and imageless) agreed in differing in effect from the motor element.

It follows that *the prevailing method of studying memory merely by means of syllables is inadequate.* Such memory involves two kinds of elements, the motor element on the one hand, and the visual, auditory and imageless elements on the other, which behave in a fundamentally different manner. Moreover, these two kinds are mixed in unknown proportions varying from individual to individual. Hence, *all previous results are equivocal.*

The learning of nonsense-syllables may prove of some value in Individual Psychology. For since in motor memory associations are equal in both directions, and in visual, auditory and imageless memories they are wholly forward, we may with ease calculate from the

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[1] Especially subject *A*, who is a poor visualiser, reported repeatedly when being examined, that a diagram came up "as a concept," *e.g.* after reacting to a colour shown he stated in the protocol that he got "a concept" and pointed out two diagrams, both of which corresponded to it. Colours, too, were revived sometimes by their saturation only, without any tint. Several times, although he learnt a series without any naming, a name was 'ecphored' in response to the stimulus. Another time he stated "Idea of bulkiness came," again "Idiopresentation, vague of form and condensed memory of aptitude of bulkiness of colour and bulkiness of figure." "All diagrams, as it were, had a meaning, were related to a system of concepts," etc.

ERRATUM

p. 463, 3 lines from bottom. The beginning of this sentence should run: "For since in motor memory associations are wholly forward, and in visual, auditory and imageless memories they are equal in both directions...."

experimental results obtained in what proportions these memories obtain in a particular subject. If both classes are equally effective then we should get 50 $^\circ/_\circ$ of the scores due to the one class, visual, auditory and imageless memories, and of these 25 $^\circ/_\circ$ would be forward and 25 $^\circ/_\circ$ backward. The other 50 $^\circ/_\circ$ would be due to the second class, motor memory, and would be wholly forward. More clearly thus:

	Forwards	Backwards
Visual, auditory and imageless memories	25 $^\circ/_\circ$	25 $^\circ/_\circ$
Motor memory	50 $^\circ/_\circ$	—
Total ...	75 $^\circ/_\circ$	25 $^\circ/_\circ$

To take the more general case, where the motor memory is a times stronger than the other class, we get the scores in the following proportion:

	Forwards		Backwards
Visual, auditory and imageless memories	1		1
Motor memory	$2a$		0
∴ forward scores : backward scores ::	$2a+1$	:	1

To take as an example subject F. We saw from the results of the experiments of group 3b that F made 14 forward scores and 5 backward ones. Hence

$$2a + 1 = \tfrac{14}{5},$$
$$a = \cdot 9,$$

i.e. the strength of the motor memory is ·9 times the strength of the other class of memories.

Before concluding I will turn to some considerations of a more speculative character. It is held as an established fact that, with the exception of Bayliss's antidromic current, Kühne's *gracilis* experiment and a few others, all intercellular nervous impulses travel in one direction only, although intracellular or nerve-trunk conduction can take place in both directions; the synapse (or according to others the cell-body) being supposed, in intercellular conduction, to act like a valve and to allow the impulse to pass in one direction only. This, so far as it can experimentally be verified by examination of any reflex arc, has always been found correct. A little reflexion will show that it probably holds good of all coordinated movements. But this cannot be verified by experiments, since we are unable to isolate the neurones or groups of neurones which on being successively discharged produce a co-ordinated movement, such as closing the open hand, or pronouncing the word 'ten.' But suppose $ABC \ldots LMN$ be the neurones in the order in which they discharge to bring about the movement in question. *A* sends an efferent current to certain muscle-fibres and an intercellular current to *B*. *B* in turn is thereby discharged sending an efferent current to other muscle-fibres and an intercellular current to *C*, and so on. Now suppose that we started by stimulating *N* and that *N* sent an efferent current to those muscle-fibres which had contracted last in the given movement, and an intercellular current to *M*, and so on, all in the reverse order. The result would be simply a series of contractions in the groups of muscle-fibres in the reverse order, producing quite incoordinate movements. What is generally held to be the reverse of a given movement, namely the opening of the hand or pronouncing the word 'net' in the cases above chosen for examples, is brought about by the contraction of quite different sets of muscles, and therefore by the discharge of totally different sets of neurones in the case of the hand; and in the case of the word, principally by the reversal of the order of discharge in *groups* of neurones. If *A* ... *N* had been the order of discharge of the neurones to produce the pronunciation of the word 'ten,' then to produce the pronunciation of the word 'net' the order would, I imagine, be something like this: *I*...*NF*...*HA*...*E*[1]. As such an incoordinate movement which would result from the discharge in the inverse order of the neurones that produce a coordinate movement is unknown to us, or at all events cannot be produced at will, we are justified in assuming that the associations of coordinate movements are irreversible.

This irreversibility is probably not confined to the associations of motor neurones, but applies to those of all efferent nerves as accelerator, inhibitory, secretory, trophic, etc., as well as vaso-motor.

If we turn now to visual, auditory and imageless processes quite different conditions obtain. We find, as shown by my experiments, that associations are equally strong in both directions, and that they are reversible. Therefore it seems probable that we have here a fundamental distinction between what I propose to call *physiological memory* on the one hand, and *psychological memory* on the other. In the former the associations are quite irreversible; in the latter they are completely reversible.

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[1] The inhibition of the tonus of the antagonistic muscles I have, for simplicity's sake, left out of account.

(*Manuscript received* 13 *December*, 1912.)

PUBLICATIONS RECENTLY RECEIVED.

Papers on Psycho-analysis. By Dr ERNEST JONES. Pp. xvi + 432. London: Baillière, Tindall and Cox, 1913. 10*s*. 6*d*. net.

The author here republishes twenty papers which have for the most part appeared in Psychological journals. Three of these papers are of a general character, five are clinical, six deal with treatment, four with dreams and two with education. Hitherto only two volumes of Freud's works have appeared in English translation. The present collection of essays, coming from so close a follower of Freud, will no doubt therefore supply a want.

The Psychology of Insanity. By Dr BERNARD HART. Pp. x + 176. Cambridge: University Press, 1912. 1*s*. net.

This is unquestionably the best modern introduction to the subject. It should be in the hands of every student of Psychology and Medicine. Technicalities are throughout avoided. The views of Freud and Janet receive judicious treatment.

A History of Psychology Ancient and Patristic. By G. S. BRETT. Pp. xx + 388. London: G. Allen & Co., Ltd., 1912. 10*s*. 6*d*. net.

Naturally the author finds a very broad definition of Psychology necessary at the outset: "History alone can adequately unfold the content of the idea denoted by the word 'Psyche' or explain the various meanings that have from age to age been assigned to the phrase 'science of the soul.'" He starts with the psychology of the early Greek philosophers. Then after dealing with the views of Plato and Aristotle, he briefly describes Indian, Egyptian, Persian and Hebrew standpoints. A comparison next follows between Hebrew and Hellenic schools of thought, after which the Pauline psychology, the psychologies of the Alexandrian school, of Origen, the Pneumatists, the Neoplatonists, Gregory of Nyssa and Augustine receive detailed treatment.

Outlines of the History of Psychology. By Prof. MAX DESSOIR: translated by D. FISHER. Pp. xxx + 278. New York: The Macmillan Co., 1912.

This work, the author explains, is not a mere abridgment of his well-known *Geschichte der neueren deutschen Psychologie*. It "covers a broader field, and... presents the development in many cases from other points of view." The book ends with a chapter on the founding of modern Psychology, reaching down to the work of Lotze and Fechner in Germany, Spencer, Tylor and Galton in England and James in America. "Since Spencer," says the author, "psychology has experienced no important changes at the hands of English investigators....Among the [English] Psychologists of the present Herbart's influence is in many ways obvious, while the experimental tendency has found relatively little encouragement" (page 238).

Die Projektionsmethode und die Lokalisation visueller und anderer Vorstellungsbilder. Von Prof. L. J. MARTIN. S. 231. Leipzig: J. A. Barth, 1912. Mk 6.

The authoress describes a long series of experiments on the projection of visual and other images in space, and on the conditions and effects of such projection.

A System of Psychology. By Prof. KNIGHT DUNLAP. Pp. xvi + 368. New York: Scribner's Sons, 1912.

A bright and stimulating little work, intended "as a main or supplementary text for semiadvanced students."

The Mechanistic Conception of Life: Biological Essays. By Dr JACQUES LOEB. Pp. 232. Chicago: University Press, 1912. $1.50 net.

A reprint of a number of essays "written on different occasions mostly in response to requests for a popular presentation of the results of the author's investigations." The first essay bears the title of the volume. The other essays of psychological interest deal with (i) the significance of tropisms for psychology, (ii) some fundamental facts and conceptions concerning the comparative physiology of the central nervous system, (iii) pattern adaptation of fishes and the mechanism of vision, (iv) an experimental study of the influence of environment on animals.

Experimental Psychology and Pedagogy: for Teachers, Normal Colleges and Universities. By Dr R. SCHULZE: English translation by Dr R. PINTNER. Pp. xxiv + 364. London: George Allen, 1912.

The chief value of this book consists in the large number of figures (314) it contains, illustrating apparatus which may be employed in the investigation of problems in educational psychology. Zimmermann's catalogue of instruments is freely drawn upon.

Ueber die allgemeinen Beziehungen zwischen Gehirn und Seelenleben. Von Prof. TH. ZIEHEN. Dritte, umgearbeitete Auflage. S. 72. Leipzig: J. A. Barth, 1912. Mk 2.20.

A new edition of the expansion of a lecture, delivered in 1901, in which the various theories concerning the seat of mental processes and their relation to the brain, from the early Greek philosophers to the nineteenth century, are discussed.

Psychologie und Wirthschaftsleben: ein Beitrag zur angewandten Experimental-Psychologie. Von Prof. HUGO MUNSTERBERG. S. viii + 192. Leipzig: J. A. Barth, 1912. Mk 2.80; geb. Mk 3.50.

In this work, the author seeks (1) to determine by experimental tests whether a given individual is fitted to adopt his chosen career in life (and thereby to modify such statistics as those relating to telephone girls about one-third of whom, apparently, are compelled to give up their work during the first half-year of their employment); (2) to inquire into the best methods of industrial work in order to produce the greatest efficiency with the least waste of energy.

Umriss einer neuen analytischen Psychologie und ihr Verhältniss zur empirischen Psychologie. Von Dr W. SCHMIED-KOWARZIK. S. vi + 318. Leipzig: J. A. Barth, 1912. Mk 7.

The author claims to treat the entire subject-matter of Psychology from the analytical standpoint instead of limiting this standpoint, as heretofore, to the consideration of those problems "which cannot be solved by empiricism, experiment and induction." As might be expected, his statements are unwittingly often in direct conflict with the results of experimental and pathological investigations.

On the Consciousness of the Universal: a contribution to the phenomenology of the thought processes. By Dr FRANCIS AVELING. Pp. x + 255. London: Macmillan & Co., 1912. 5*s.* net.

A thesis, approved for the Degree of Doctor of Science in the University of London, and consisting of three parts. "The first briefly sets out the history of the development of the problem of the 'Universals' with its metaphysical, epistemological, and psychological implications. The second presents the data of our present research with the conclusions thereon based. The third consists of a short Bibliography of works referred to in the text." *Inter alia*, the author concludes from his experiments that "images are not necessary as contents for thought; but thinking always involves concepts as contents"; that "the 'universal' is phenomenologically present, or tends to be present, to consciousness as a concept or imageless substantive content"; that "the 'individual' is phenomenologically present to consciousness as a concept in connection with sensorial contents (image)."

Correlations of Mental Abilities. By Dr **Benjamin R. Simpson.** Pp. iv + 122. New York City: Teachers College Series, 1912. $1.00.

This monograph, undertaken under Prof. Thorndike's guidance, "is a contribution to knowledge of individual differences, especially in that complex of qualities which we call 'general intelligence,' and of the means of measuring them. Its special purpose is to determine the *significance* of certain tests by showing what relations they have to one another and to general intelligence in its common meaning" (page 3). The author finds "justification for the common assumption that there is close inter-relation among certain mental abilities, and consequently a something that may be called 'general mental ability' or 'general intelligence'; and that on the other hand certain capacities are relatively specialized, and do not necessarily imply other abilities except to a very limited extent" (page 109). But he finds "no justification for the view that 'general intelligence' is to be explained on the basis of a hierarchy of mental functions, the amount of correlation in each case being due to the degree of connection with a common central factor" (page 110).

Psychologie und Erziehung: Ansprachen an Lehrer. By **William James**: translated by Dr F. **Kiesow.** S. 134. Leipzig: W. Engelmann, 1912. Mk 1.

The Dynamic Foundation of Knowledge. By Dr **Alexander Philip.** Pp. xii + 318. London: Kegan Paul, 1913. 6*s.* net.

La Ilusión. Por Prof. **Carlos R. Etchart.** Pp. 253. Buenos Aires: Coni Hermanos, 1913.

Psicología Energética: lecciones dictadas en la facultad de filosofía y letras. Por Prof. **Carlos R. Etchart.** Pp. 94. Buenos Aires: Coni Hermanos, 1913.

Principios de Psicología Biológica. Por Prof. **José Ingenieros.** Pp. 471. Madrid: Daniel Jorro, 1913.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

November 2nd, 1912.	Association and Memory, by A. **Wohlgemuth.**
	New Apparatus for the Examination of Cutaneous Temperature Spots (Demonstration), by A. **Wohlgemuth.**
	Experience of Pain, by **Carveth Read.**
	Reminiscence, by P. B. **Ballard.**
January 25th, 1913.	Reminiscence (*concluded*), by P. B. **Ballard.**

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